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2021

When the Tables Turn: Marriage and Gender Inequality in Latin America

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Princeton, NJ : Princeton University

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When the Tables Turn: Marriage and Gender Inequality in Latin America

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A Dissertation Presented to the Faculty of Princeton University in Candidacy for the Degree of Doctor of Philosophy

Recommended for Acceptance by the Department of Sociology

Adviser: Dalton Conley

May, 2021

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Abstract

In the last few decades, Latin American women had dramatically increased their educational attainment and economic position relative to men. However, these transformations unfolded amidst the prevalence of cultural norms—including gendered views of the division of labor and hypergamy ideals in marital selection—that are in tension with the reduction of these gender gaps. This dissertation examines whether, under these contextual conditions, increases in women's status reduce long-standing gender inequalities in families. To do so, I focus on two policies driving some of these transformations in the Latin American context—conditional cash transfer programs and educational expansion reforms.

The first chapter examines conditional cash transfers as a case where women's economic position is drastically improved via welfare payments. In particular, I analyze Mexico's Progresa program, which provided cash transfers to low-income women following an experimental design. I find that these payments shifted husbands' attitudes towards a more equitable division of labor and improved wives' autonomy. Notably, these transfers did not generate a gender backlash response among treated families—such as pushing women further into unpaid labor—as gender-based theories posited.

In the second chapter, I analyze whether increases in women's schooling reduce gender asymmetries in marriage in Bolivia, Colombia, and Peru. I show that in two dimensions of union formation—marriage entry and marital selection—status differences between men and women prevail as gender gaps in education are reduced. My results demonstrate that highly educated women

are less likely to get married in all countries and that marriage propensities favoring hypergamy persist across cohorts, confirming the endurance of gender asymmetrical norms.

In my last chapter, I inspect whether mass education reforms increase women's autonomy and decision-making—a classic argument among demographers and development scholars. By leveraging the timing of compulsory schooling reforms in Bolivia, Colombia, and Peru, I show that while education is a means of empowerment for most women, further schooling is an ineffective tool to increase female autonomy among rural and indigenous populations. I argue these results are explained by changes in the selection into schooling and the effects of women's education on marriage.

Acknowledgments

I have many people to thank. During these six years at Princeton, I had the chance to learn and be supported by a fantastic group of scholars. Dalton Conley was from the beginning receptive to my ideas, guiding me through complex methodological problems, and giving me the freedom I needed to develop my research agenda. I am very grateful for his time, feedback, and support. I have been fortunate to have Viviana Zelizer as one of my mentors, being a constant source of inspiration and encouragement. I especially thank her for pushing me to think about the broader sociological implications of my work. Maggie Frye has taught me so much about the craft of research by co-authoring together and receiving her insightful feedback about my work. I am grateful for having Maggie's generous support and mentorship throughout the years. I would also like to thank Jen Jennings for encouraging me to "own" my expertise. Our meetings always left me motivated to keep doing the work and enjoying the process. I'm also grateful for Arun Hendi's thorough feedback and demographic advice, which I greatly needed. Lastly, I thank Jim Raymo for providing me with critical guidance in the last stride of my dissertation.

My life as a graduate student has been enjoyable and unforgettable, thanks to many friends. Jeremy Cohen, Janet Xu, and Simone Zhang have read almost every piece of work I've written during my Ph.D.; my growth as a scholar would not have been the same without them. Beyond that, I thank them for their friendship and their support. I am also grateful for sharing this experience with Parijat Chakrabarti, Yo-yo Chen, Gracie Himmelstein, Galileu Kim, Herrissa Lamothe, Ian Lundberg, José María Rodriguez, and Elsa Voytas, filling my graduate school days with memories

of fun conversations over delicious home-cooked meals. I also thank Mauricio Bucca for his friendship and academic guidance since I was an undergraduate student in Chile.

I am especially thankful for my family and their continuous support since I decided to move to the U.S for graduate school. My parents, Juan Carlos and Gloria, taught me the value of hard work and education, and for that, I'll always be grateful. My siblings Claudia, Andrea, and Sebastián, have been a constant source of companionship and optimism as we all strive to achieve our goals. Finally, I thank my partner Pablo Argote for embarking on this journey together and supporting me in every way possible. From our heated debates in undergraduate classrooms to his last comments about this dissertation, Pablo has critically engaged with my ideas, making me a better scholar because of it.

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Chapter 1

Introduction

Within-household inequalities have been a central topic in sociological research. Not long ago, gendered disparities in decision-making, care responsibilities, and household labor, somewhat reflected the status differences between men and women in the broader social structure. Indeed, women's ability to get an education, participate in paid employment, elect political representatives and own property were still limited in most countries (Goode 1963; Therborn 2004). But in recent decades, women's educational and economic standing started to dramatically improve across the world (DiPrete and Buchmann 2013; Grant and Behrman 2010), shifting the gendered access to financial resources and *potentially* reducing inequalities in the realm of families and romantic relationships (England 2010; Charles 2011).

Latin America has been no exception to these changes. For instance, several countries in the region display a female advantage at all levels of instruction, and a handful of others are on the verge of closing all gender gaps in educational attainment (Esteve et al. 2016; Grant and Behrman 2010). Nevertheless, these shifts in women's status have taken place amidst the prevalence of cultural norms—including gendered views of the division of labor in families, the expectation of submissive behavior from women, and hypergamy ideals in marital selection (Chant 2002; Orgill and Heaton 2005)—that in many ways are in tension with the reduction of these gender gaps.

This dissertation examines whether, under these contextual conditions, increases in women's status reduce long-standing gender inequalities in family dynamics and union formation. To do

so, I focus on two major policies driving some of these transformations in the Latin American context—conditional cash transfer programs and educational expansion reforms. I leverage specific features of these policies to causally identify or thoroughly describe the effects of reducing status differences between men and women on union formation processes, the division of housework, and female autonomy. This dissertation is structured in three stand-alone chapters. Each chapter answers a particular research question, engages with different hypotheses in the literature, and applies a distinct analytical strategy. I now turn to provide an overview of each dissertation chapter.

The dissertation begins in **Chapter 2** by examining a long-standing question in the sociology of the family—what is the effect of women's economic resources on the division of housework and decision-making power? Although scholars have extensively studied the relationship between women's financial resources and household dynamics, most of this research has focused on the effects of labor market earnings (Brines 1994; Lundberg and Pollak 1993; Killewald and Gough 2010). As economic sociologists have demonstrated that money is not a uniform, fungible quantity, but instead is routinely differentiated by individuals (Sykes et al. 2015; Zelizer 1994), whether welfare payments can increase women's position in the home is an important question. To this end, this chapter focuses on the effects of *welfare payments* on gender inequalities in the home. In particular, I study the case of Mexico's Progresa program, which provided cash transfers to low-income women following an experimental roll-out, offering a unique opportunity to estimate the causal effects of these payments on household dynamics. These programs have been a landmark policy of Latin American governments seeking to reduce income inequality and promote women's economic empowerment (Parker and Vogl 2018).

My main findings show that these payments shifted husbands' attitudes towards a more equitable housework division and improved wives' autonomy. Notably, these transfers did not generate a gender backlash response among treated families—such as pushing women further into unpaid labor—as gender-based theories posited (Brines 1994; Greenstein 1996; Molyneux 2006). These results attest to the power of economic resources for improving women's position in the house-

hold, further confirming the claims of resource-based theories in sociology and economics (Cook and Whitmeyer 1992; Lundberg and Pollak 1996). Finally, I argue that two features of Progresa's design were especially relevant for observing these results—the fact that mothers had to fulfill monthly conditions to receive the cash transfers and the State's framing of this program as a policy for "female empowerment"—which infused these payments with meanings of effort and fairness. Overall, this chapter illustrates how we can exploit field experiments to answer sociological questions concerning the family, a strategy that could be increasingly fruitful given the prominence of randomized control trials in the policy arena. Lastly, a version of this chapter was published in the *Journal of Marriage and the Family*.

In **Chapter 3**, I turn to examine the implications of increases in women's educational status relative to men. Although the rise of female schooling is a worldwide trend, the highest growth rates in the Global South are found in Latin America (Grant and Behrman 2010). As noted earlier, in most Latin American countries, women had equalized or surpassed men's educational attainment (Esteve et al. 2016). These changes in educational composition portend shifts in marriage processes, considering the long-standing gender asymmetries in union formation in Latin America. In most of these countries hypergamy norms—the cultural expectation that men should marry down and women should marry up in educational attainment, income, and other social status markers (Basu 1992; Bourdieu 2007)—still structure marriage markets. Generally, unions in which wives have more schooling than their husbands are perceived as "gender-deviant" (Chant 2002; García and de Oliveira 2011), and women in these relationships are more at risk of experiencing intimate partner violence and reductions in their autonomy (Friedemann-Sánchez and Lovatón 2012; Menjivar 2011).

Under these contextual conditions, does the increase in women's schooling relative to men reduce enduring gender asymmetries in union formation? To answer this question, I focus on Bolivia, Colombia, and Peru, which present varying degrees of gender gaps in education. While Colombia displays female educational advantages at all levels of schooling, in Bolivia and Peru, recent cohorts just reached educational parity (IPUMS 2020). In particular, I investigate how changes in

educational composition influence two dimensions of union formation: marriage entry and marital selection or "who marries whom." To this end, I combine both DHS surveys and national censuses and rely on discrete-time hazard models and log-linear analyses to assess marriage timing and marriage selection, respectively.

These analyses reveal two key findings. First, I found that highly educated women were less likely to get married and that this negative relationship increased across cohorts in all countries. Second, trends in marital pairings show less change towards gender symmetry than expected. While in Colombia we observe a decline in hypergamy and a rise in hypogamous unions (where women have more education than their husbands), these shifts were mostly explained by changes in education composition and not by transformations in marriage propensities—social norms, preferences, and gendered ideals guiding the desirability or rejection of specific marital matches (Schoen and Kluegel 1988). Furthermore, Bolivia and Peru display an increase in the proportion of hypergamous marriages—an unexpected finding given the closure of gender gaps in both countries. Overall, this evidence highlights the persistence of gender asymmetrical norms in union formation. The little sign of change in marriage propensities in Colombia, after decades of female advantages, suggests it may be a while before we observe more profound transformations in these dynamics.

Lastly, **Chapter 4** focuses on the implications of women's increased education for their autonomy. Schooling, especially in the Global South, has been promoted as an effective policy to enhance women's empowerment. Indeed, scholars and international organizations alike have asserted that educational reforms would accelerate family change by increasing women's status in the home—promoting their autonomy and decision-making (Caldwell 1976; Mason and Bumpass 1975; Murphy-Graham and Lloyd 2016). Although several studies show that education is associated with women's greater autonomy (Acharya et al. 2010; Oropesa 1997), others find its nonconsequential (Heaton et al. 2005; Malhotra and Mather 1997), and there is still scant evidence that causally identifies this relationship. This paper addresses this challenge by leveraging the timing of compulsory schooling reforms in three Latin American countries: Bolivia, Colombia, and Peru. Using Demographic Health Surveys, I implement an instrumental variable design using random

exposure to compulsory schooling laws as an instrument for years of education.

In my analyses, I find there is a positive association between women's schooling and greater autonomy. However, as educational systems expand, these returns progressively diminish in magnitude. Once I address endogeneity via instrumental variables, I show that women who complied with compulsory laws and gained further education experienced a small decrease in their autonomy in all countries. Additional results confirm that women complying with these reforms in Bolivia and Peru were more likely to come from rural backgrounds and identify as indigenous. These findings suggest that while for most women, education enhanced their autonomy, for those who were less likely to continue their education in the absence of compulsory reforms, further schooling did not fulfill the promise of greater autonomy.

Overall, I interpret these findings as consistent with two non-exclusive explanations for compulsory schooling's inability to increase women's autonomy. One concerns the trade-off between school expansion and student selectivity. A secondary explanation relates to the interaction between women's education and union formation patterns. In particular, I argue that compulsory schooling in contexts where women's education is viewed as a threat to marital norms may generate the unexpected consequence of curtailing married women's autonomy. Together, these results highlight the importance of examining the returns to mass schooling considering population heterogeneity and the contextual meaning of women's education.

In **Chapter 5**, I present concluding remarks, elaborating on my findings across chapters, and provide a roadmap for future work.

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Chapter 2

In the Hands of Women: Conditional Cash Transfers and Household Dynamics in Mexico

Introduction

Over the last decades, many policy initiatives in the developing world have focused on improving women's economic conditions as a strategy to reduce poverty and increase gender equality—examples of these programs are micro-credits, conditional cash transfers and rotating credit and saving associations (Moser 1989; Sanyal 2009). Yet the extent to which these programs actually improve women's status in the household remains an open debate (Molyneux 2006; Nagels 2016). Although social scientists have extensively studied the relationship between women's economic resources and household dynamics, most of this research has focused on the effects of women's labor market earnings (e.g., Brines 1994; Bittman et al. 2003; Cunningham 2007; Greenstein 2000; Gupta 2007; Weitzman 2014a). How other sources of money—in particular welfare payments—affect women and within-household inequality has been less explored.

Studying the implications of welfare money on household dynamics is relevant for both theoretical and policy reasons. Both sociologists and behavioral economists have demonstrated that money is not a uniform, fungible quantity, but rather is routinely differentiated by individuals who assign different meaning and uses to particular types of money (Thaler et al. 2000; Sykes et al.

2015; Zelizer 1994). In other words, as "not all dollars are equal" (Zelizer 1989: 343), whether and under what conditions welfare payments are able to increase women's decision-making or reduce their housework time is an important question.

To fill this research gap, this study asks two research questions: Do welfare payments improve women's position in the household? Do these payments generate compensatory actions from husbands in the home? I answer these questions in the context of Mexico's Progresa program, a conditional cash transfer (CCT) provided to low-income mothers in rural villages. A particular feature of this policy was that mothers had to fulfill monthly program conditions, such as ensuring their children attended school and regular health checkups, to receive the cash transfers. The case of Progresa offers the unique opportunity to assess the causal effects of cash transfers on household dynamics due to its experimental design.

In this chapter, I focus on the impact of these payments on three outcomes: intra-household decision-making, female autonomy, and perceptions of responsibility for housework. I extend resource-based, time constraints, and gender-based theories to the case where women's income is mostly the product of a welfare program. Resource-based theories predict that conditional cash transfers should improve women's situation in the household (Cook and Whitmeyer 1992; Lundberg and Pollak 1993), while time constraints approaches assert this effect depends on the time spent in income-generating activities (Blood and Wolfe 1960). In contrast, gender-based theories emphasize that these payments might lead to the reinforcement of traditional gender roles, as a compensation for the transgression of gendered economic expectations (Brines 1994; Greenstein 2000).

To examine these hypotheses, I use data from the National Survey of Household Characteristics (ENCEL) and focus on a sample of married and cohabiting couples participating in Progresa (N=9,751 unions). In particular, I estimate both intent-to-treat and local average treatment effects of Progresa payments after 1.5 years. Findings indicate that Progresa cash transfers had a *moderate* positive effect, circumscribed to attitudinal measures of housework and female autonomy, while

they did not affect household decision-making. Yet, these payments did not generate a gender backlash response among treated families—such as pushing women further into unpaid labor, as some critics expected. Together, this study enhances scholarly understandings of the relationship between women's economic resources, targeted public policies, and household inequalities.

Theoretical Frameworks and Hypotheses

To generate hypotheses on the ways in which a cash transfer program like Progresa could affect intra-household decision-making, female autonomy, and perceptions of responsibility for housework, I draw upon resource-based (Blood and Wolfe 1960; Cook and Whitmeyer 1992), time constraints, and gender-based theories (Brines 1994; Greenstein 1996; West and Zimmerman 1987). Although previous work has applied these frameworks to the case of labor market earnings, I apply these theories to the case where women's income is mostly the product of a welfare program.

Resource-based Theories

A series of approaches hypothesize that wives' economic power affect their position in the household. The autononomy perspective posits that wives' *absolute earnings* decreases their housework time and increases their overall autonomy, via the outsourcing of household tasks, for example (Gupta 2006, 2007). More generally, this perspective includes any mechanism that explains the relationship between spouses absolute earnings to lower contributions to household labor and higher autonomy. In the case of welfare payments, it may be that once wives receive this new source of support, they feel more empowered to make household decisions (Gupta 2006, 2007). Thus, extending this hypothesis to the case of Progresa payments we should expect that:

> **Autonomy hypothesis**: Receiving a welfare payment will lead to a reduction in the attribution of housework tasks to wives, as well as an increase in their autonomy and household decision-making.

Building on this framework, scholars have further hypothesized that the relationship between women's earnings and housework is one of diminishing returns (Hook 2017; Killewald and Gough 2010). Although this hypothesis has only been tested for the case of household labor, we can hypothesize a similar relationship for related outcomes:

> **Marginal returns hypothesis**: Welfare recipients that are more economically dependent on their husbands at baseline, will experience greater positive returns to these payments in terms of the attribution of housework tasks, their decision-making and autonomy. This compared to recipients that are less dependent on their spouses.

Alternatively, another group of scholars has focused on the role of *spouses relative earnings* on household dynamics. This framework—known as resource-exchange theory—has a long tradition in sociology and economics (Blood and Wolfe 1960; Cook and Whitmeyer 1992; Lundberg and Pollak 1993). In this view, the contribution of economic resources compared to one's spouse is what determines the level of dependency and bargaining power in a marriage. Therefore, increases in wives' incomes *relative* to their husbands', would improve their bargaining position to make decisions and reduce their housework time, while also generating a compensatory response from spouses in the form of greater contributions to house chores or the deferral of household decisions.

Following this perspective, sociologists have argued that welfare payments provided to women increase their relative power by enhancing their capacity to maintain an autonomous household (Cooke 2007; O'Connor 1993; Orloff 1993), and thus, being able to credibly threat to leave their marriage (Lundberg and Pollak 1993). In contexts where divorce is unlikely, such as the case of rural Mexico (Mexico Census 2000), dyadic power is established via the ability to provide and withhold these welfare resources (Lundberg and Pollak 1993). Two studies have assessed resource-exchange dynamics in the case of welfare payments. First, Lundberg, Pollak and Wales (1997) examined the implications of transferring the UK's child allowance program to wives for income allocation decisions. The reform was associated with a substantial shift towards greater expenditures on women's and children's goods, suggesting the allowance had an impact on wives'

decision-making power. Second, Cooke (2007) found in the U.S a positive association between women's share of welfare income and their husbands' participation in housework. Together, this perspective leads to the following prediction:

> **Resource-exchange hypothesis**: Being recipient of a welfare payment will improve wives' position in the household—reducing the attribution of housework tasks to wives and increasing their decision-making and autonomy—while also leading to a compensatory response from spouses in these same areas.

Lastly, note that in resource-exchange wives' gains in household dynamics necessarily imply a "compensation" response from their husbands that is not expected in the autonomy hypothesis. For instance, we expect that husbands not only attribute less housework tasks to their wives, but also increase the number of chores that they perceive as their responsibility.

Time Constraints Perspective

In contrast to a focus on labor market earnings, this approach argues that the time allocated to domestic chores is shaped by the time each spouse spends working outside the home (Blood and Wolfe 1960). Individuals are expected to do more housework the less time they spend on paid work, and the more hours their partners spend on employment. Scholarship in industrialized countries have found that wives' employment and work hours are associated with a decrease of her housework time (Fuwa 2004; Knudsen and Waerness 2007; Mannino and Deutsch 2007), and an increase of their husbands' housework time (Bianchi et al. 2000; Cunningham 2007; Hook 2017). Moreover, several of these studies have found that *both* wives' earnings and paid work hours are explanatory factors for the division of household labor (Bianchi et al. 2000; Fuwa 2004; Knudsen and Waerness 2007; Hook 2017). Suggesting that resource-based and time constraints are more complementary than exclusionary hypotheses. Although the time constraints perspective does not directly refer to how individuals change their attitudes towards housework, it is reasonable to think

that time constraints change both the actual division of labor and the *perceptions* of responsibility attached to those tasks.

In the case of Progresa payments for which wives have to fulfill time-demanding requirements, the time constraints perspective leads to the following hypothesis:

> **Time constraints hypothesis**: Being recipient of a welfare payment with time-demanding requirements, will reduce the attribution of housework tasks to wives, and increase the number of chores husbands perceive as their responsibility.

The logic behind this hypothesis is that welfare programs that require time outside the home—such as the case of conditional cash transfers—will generate a proportional shift in the perceptions of housework responsibilities between spouses to make up for these time constraints. However, the theoretical processes concerning the effects of time availability on household decision-making and autonomy are less clear, so I do not generate a priori predictions for these outcomes.

Gender-based Theories

Finally, gender based theories are another major approach in the study of economic resources and household dynamics. In this view, gender shapes power between couples not only through the economic resources each spouse can bring to the table, but also via the gender expectations attached to specific behaviors in everyday interaction (West and Zimmerman 1987). Precisely, gender deviance neutralization theory argues that when gender expectations—such as the male breadwinner ideal—are transgressed, couples respond by reinforcing traditional gender roles in other spheres (Brines 1994; Bittman et al. 2003; Greenstein 2000). This would explain why wives who out-earn their husbands still end up doing more housework, for instance.

Although gender deviance neutralization was empirically supported for the case of housework in Australia (Bittman et al. 2003), the U.S (Brines 1994; Greenstein 2000), and Sweden (Evertsson

and Nermo 2004), more recent analyses have concluded that the original identification of these studies was questionable. Some scholars argue these previous findings were limited to a very small group of men that earned significantly less than their wives (Sullivan 2011), and also were partly a product of not controlling by women's absolute income (Gupta 2006, 2007; Hook 2017). Both issues cast serious doubt on the analytical and empirical leverage of gender deviance neutralization arguments in gender egalitarian contexts.

Nevertheless, this approach has found significant empirical support in developing countries, where women have drastically improved their economic and educational status amidst pervasive patriarchal norms (World Bank, 2007). For example, in India (Weitzman 2014a), Bangladesh (Naved and Persson 2005), Nicaragua, and the Dominican Republic (Kishor and Johnson 2004) women's higher share of earnings are associated with higher physical and/or sexual violence from their spouses. Similar findings have been for housework participation in India (Luke et al. 2003). In light of this evidence, I hypothesize that wives suddenly receiving a non-negligible amount of money, via a welfare payment, could lead to "gender backlash" responses due to the transgression of traditional gender norms. This might be especially the case in less egalitarian countries where patriarchal norms are pervasive, such as rural Mexico (García and de Oliveira 1994). In fact, gender backlash responses were a concern policy and gender scholars often made when referring to the unintended consequences of conditional cash transfer programs like Progresa (Molyneux 2006; Nagels 2016). Together, gender-based theories applied to the case of welfare payments posit that:

> **Gender backlash hypothesis**: Welfare payments will lead to the reinforcement of traditional gender roles to compensate the transgression of gender expectations. In particular, we should expect an increase in the attribution of housework tasks to wives, as well as a reduction of their autonomy and household decision-making.

Note that this hypothesis assumes that the gendered meanings of earned and welfare money

are similar, to the extent that both sources of income are perceived as a threat to the male breadwinner ideal.

Research Setting

Policy Background: The Progresa Program

Conditional Cash Transfer (CCT) programs have become a widely implemented policy in developing countries since the 1990s. One of the first and most popular CCTs was Mexico's Progresa program. This policy was implemented in 1998, first in rural villages, and later expanded to cover more than 30 million poor Mexicans (Behrman and Parker 2013). In fact, the program is still in operation under the name of "Prospera" (Parker and Vogl 2018). After studies demonstrated Progresa had positive effects on outcomes such as children's education and adult's health (Behrman and Parker 2013), CCTs became one of the main policy tools to aid the poor in Latin America, as well as in a number of countries in Asia and Africa, and even the United States (Díaz-Cayeros and Magaloni 2009; Parker and Vogl 2018).

A key feature of Progresa's design was the delivery of a cash transfer directly to mothers with school-age children. The cash transfer was provided every two months and composed of two parts, a flat transfer and a schooling transfer, which varied according to the number, gender and age of children (Handa et al. 2009). To get the cash transfer beneficiaries needed to fulfill two conditions: (1) Enrolling their children in school and ensuring their regular attendance and, (2) attending health clinics for checkups (Parker and Vogl 2018). Every two months, beneficiaries were required to personally collect their payments at the closest assigned office; this process usually took a whole day between the travel time and paperwork (Luccisano 2008). The program also offered basic health care and health workshops for all family members. Most importantly, Progresa represented a substantial increase of households' economic resources—a 22% rise of household income and a 97% increase of women's income, on average (Adato et al. 2000; Author's own calculations,

Progresa Data 1997-1999). In 2000 the average family received $28.50 USD/month (Behrman and Parker 2013).

Gender Context and Recipients' Accounts of the Progresa Cash Transfer Program

Progresa was the first national anti-poverty policy in Mexico where women took a central role (Luccisano 2008; Molyneux 2006; Nahmad et al. 1999). Government officials expected that by making mothers the recipients of these welfare payments, the program would be more successful in improving the opportunities and the quality of life of poor children. Indeed, in government discourse women were portrayed as being more responsible and capable than men when it came to household money and families' needs (Medina H. 2001). And ultimately public officials stated that one of the aims of Progresa was to "empower" low-income women (Luccisano 2008).

Given the gender context in rural Mexico, the government took an important risk in positioning mothers as the forefront actors of the program. At the time of Progresa's implementation, gender expectations in rural areas were especially traditional. In most families husbands held the position of central authority and main decision-maker (García and de Oliveira 1994; González Montes 1991; Stern 1999), and women usually had to consult them to leave their house or make household purchases (García and de Oliveira 1994). The male breadwinner ideal and the belief that women should avoid paid work and devote themselves to domestic chores were ubiquitous; directly conflicting with Progresa's design (De Barbieri 1984; Romero Rabell 2009). These gendered norms were also illustrated in women's low labor market participation. In 2000 only 22 percent of rural women between the ages of 16-64 participated in the labor force, compared to 44 percent of women in urban areas (Angoa-Pérez and Fuentes-Flores 2006). Finally, women's ability to leave abusive marriages was restricted. Rural women tended to marry at a young age—22 years old, on average— and divorce, although legally existent since 1917, was relatively rare. In 2000, the national rate of divorce for women was only 2.8 percent (ages 40-44) (Mexico Census 2000).

In line with the government's discourse, a consistent finding of qualitative assessments of Progresa is that recipients perceived the program as a signal of recognition. For instance, in the International Food Policy Research Institute's (IFPRI) evaluation, the majority of recipients talked about how the program recognized that women are more responsible towards money and family than men (Adato et al. 2000). Other qualitative assessments show that recipients felt respected because the government trusted in their capacities to appropriately administer Progresa money (Medina H. 2001; Nahmad et al. 1999). Meanwhile, women generally revealed their husbands did not have problems with the program (Adato et al. 2000; García Falconi 2010; López Rivera 2003). That said, a smaller number of recipients reported family conflict as a result of Progresa. Most of these incidents seem to occur either because husbands wanted to manage welfare money, or because they were upset with their wives' time commitment to the program (Adato et al. 2000; García Falconi 2010; López Rivera 2003).

Methods

Sample, Balance and Compliance

For this analysis I use the National Survey of Household Characteristics (ENCEL) collected by the Mexican government to evaluate Progresa. Specifically, I use the baseline questionnaires (1997) and the 1999 follow-up surveys that include a special module on household dynamics, including housework attitudes and decision-making measures. The ENCEL data also contains information on spouses' earnings, education, program transfers and other socio-demographic characteristics before and after Progresa. I restricted the sample only to married or cohabiting couples (I will use the term wife/mother and husband/father to refer to both types of unions) with children younger than 18 years old and classified as poor by the Mexican government—two conditions necessary to be eligible for the program. This yields a total sample of 9,751 unions, where 6,139 are in the treatment group and 3,612 are in the control group. The attrition rate between the baseline survey and follow-up questionnaires was of 16%. In order to control for potential attrition bias, baseline

individual and household characteristics that could be determinants of attrition are included in all models (Bobonis 2009).

The initial roll-out of Progresa had a randomized control trial design with an elaborate targeting mechanism. The initial experiments were implemented in villages selected from seven states (De la O 2013). The first step was to select the program villages according to an aggregated poverty index constructed from census data; 506 villages where randomly assigned to Treatment (320) and Control (186). Within these villages the program was offered to all households classified as poor by the National Survey of Household Characteristics. The take up was extremely high, with 97 percent of eligible families participating (Parker and Vogl 2018).

The overall randomization of villages to treatment and control was assessed by Behrman and Todd (1999), who concluded that there were no systematic pre-treatment differences between the two conditions. This ensures that control villages are a valid counterfactual for the treated units. Nevertheless, I also check if pre-treatment covariates at the household and individual level are balanced between treatment and control groups in my sample. For this, I run a series of univariate two-sided t-tests for all pre-treatment covariates (Gerber and Green 2012). Some variables, such as labor force participation, having a TV and owning a house, do not pass this balance test. However, as can be seen in 1, the mean differences between treatment and control are very small. All variables with a significant p-value in these t-tests are included in the vector of controls, to increase estimation precision (Gerber and Green 2012).

[Table 1 here]

In addition, in field experiments it is often the case that not all units originally assigned to treatment actually *receive* the treatment; this issue is also known as "one-sided non-compliance". Although almost all families assigned to Progresa agreed to enroll in the program, 19% of families did not receive the treatment, in other words, they got zero cash installments in the observed period. To take this issue into account I estimate both intent-to-treat (ITT) estimates—which esti-

mate the effect of the treatment on the assigned units—and local average treatment effect estimates (LATE)—which gauge the effect of the treatment on the treated. Additional details of these estimands are provided in the Analytical Strategy.

Attrition

An important issue is the extent of attrition in the data. If being out of sample is correlated with the probability of receiving treatment, this could lead to bias in the estimates of program effects. The attrition rate between the baseline survey and follow-up questionnaires was 11 percent, and was relatively balanced between treatment (10%) and control groups (12%). Although the precise cause of attrition is unknown, prior work has suggested that most of it was due to household residential mobility (Bobonis 2009). Table 9 in the Appendix contains descriptive statistics comparing attriters versus non-attriters at baseline. To analyze more systematically whether attrition is related to treatment assignment or other observables, I estimated an OLS regression where the dependent variable is attrition. This model indicates that treatment assignment alone does not predict being out-of-sample, but some characteristics such as owing a radio or being married predict attrition in the control group (See Table 10 in the Appendix).

In order to address the possibility of nonrandom sample attrition, I follow two procedures. First, I include individual and household characteristics that could be factors of nonrandom attrition in all estimation procedures. Second, in order to show how attrition could potentially affect Progresa estimates, I follow the nonparametric method by Lee (2009) to calculate extreme bounds on program effects when there is differential attrition across treatment and control groups. This approach yields a set of best and worse case-scenario bounds by identifying the excess of observations that leave the sample and then "trim" the size of the group with less attrition by this number (See Lee 2009). Bounds for ITT estimates without covariates were computed via the Stata package *leebounds*. Given that attrition rates are not high, bounds are relatively tight and do not significantly deviate from the main results of this study.

Measures

Despite that the ENCEL data contains detailed information of both spouses, the questions pertaining to household dynamics were responded by husbands. Previous scholarship has found gender and class biases in survey responses on the division of household labor and decision-making (e.g., Kan 2008). Although this is an important limitation of the present study, this should not bias Progresa treatment estimates for two reasons. First, given that families were randomized into experimental groups, on expectation response-bias should be equally distributed across treatment and control (Gerber and Green 2012). Therefore, we can still adequately estimate treatment effects between the two conditions. Second, prior work has found that working class men, such as those in Progresa, tend to under-report their role in housework (Kan 2008). This suggests that husbands responses will tend to yield conservative estimates of the impact of Progresa cash transfers.

All measures used in this analysis were collected 1.5 years after the start of the Progresa program.

Dependent Variables

All measures used in this analysis were collected 1.5 years after the start of the Progresa program. My first outcome of interest is the perceptions of responsibility for housework. The ENCEL survey contains several questions asking who should perform the following tasks: (1) do laundry, (2) carry water, (3) take care of animals, and (4) take care of the children. Its important to note that child care is usually not conceptualized as household labor by resource-exchange theories, which assume that partners do not want to opt-out from this responsibility (Sullivan 2013). Nevertheless, as child care is very much considered a female task in rural Mexico (García Falconi 2010), I still included this measure as an indicator of household labor, but keeping in mind that it could be less responsive to welfare cash transfers. For each task respondents may answer that (i) the father, (ii) the mother, (iii) the sons, (iv) the daughters or (v) everyone should be in charge of the task.

The majority of these tasks are traditionally female duties in rural Mexico (García Falconi

2010). From this question I generated two measures. First, whether the mother is supposed to do each task (1) versus anyone else (0). Second, whether the father is supposed to do each task (1) versus anyone else (0). I also checked alternative coding schemes to explore if they were subtler shifts in the allocation of responsibility for housework, including changes for daughters and sons. None of these alternative schemes yielded important results in terms of magnitude or statistical significance.

My second outcome is intra-household decision making. The ENCEL includes a number of questions asking who (1) decides when to take the children to the doctor, (2) decides whether their child should go to school if they do not want to, (3) decides how much to spend in children's clothing, (4) decides how much to spend in household repairs. For each item respondents may answer that the main responsible for this decision is (i) the father, (ii) the mother, (iii) both of them, (iv) other man in the household, or (iv) other woman in the household. I created two measures from these set of questions. First, whether the mother takes each decision (1) versus everyone else (0). Second, whether the father takes each decision (1) versus everyone else (0). As a robustness check, I also analyzed whether the mother takes the decision (1), versus her husband (0). Additionally, I tested coding strategies focused on shared decision-making, but none of these alternative schemes yielded important results in terms of magnitude or statistical significance.

Measures for housework attitudes and intra-household decision making are somewhat crude and might lead to conservative results. This is partly because they are absolute measures and do not show marginal changes that could be taking place. For instance, it might be the case that the father does not perceive doing laundry as his responsibility, but after the program considers that he should contribute a couple of hours to fold clothes. Moreover, the construction of the original questions does not allow for plausible combinations of housework attribution, such as "mother and daughter" or "all children".

Finally, I also assessed three measures pertaining to **female autonomy**. First, respondents are asked "When your wife needs to visit a relative or a friend, does she need to ask you for

permission?" I generated a measure equal to (1) if the response was that she did not have to ask for their husband's permission and equal to (0) if they had to. Second, respondents are also asked: "When your wife needs to go out to visit someone, who goes out with her?" This question was coded equal to (1) if the respondent answers she is able to go out alone and (0) if she either needs to do so with someone else or she never goes out alone. Third, respondents are also asked who *should* administer a woman's cash. The given options are (1) she should administer it, (2) she should give it to her husband and (3) both should administer it. Likewise, I generated a dummy variable coded (1) if they think the woman itself should administer her cash versus not (0), and another measure equal to (1) if they think the husband should administer it versus not (0). Within female autonomy indicators, this is the only attitudinal outcome.

Besides estimating the effect of Progresa cash transfers on each of these measures, I also created an index for each dimension of household dynamics—housework attribution, decision-making, and female autonomy. These indexes were created to analyze how cash transfers affect household dynamics more broadly. For their construction, I used the measures above that focused on wives and standardized them for the creation of each scale. This procedure yielded three indexes with varying levels of consistency, spanning from low levels of reliability in the case of the index of female autonomy (0.25), to more acceptable levels in the case of the housework attribution index (0.60), and the decision-making index (0.49) (Cortina 1993). Given their varying levels of consistency, the impact of cash transfers on these indexes should be interpreted together with the findings pertaining to specific measures of household dynamics.

Explanatory Variables

First, I generated a **treatment assignment** variable equal to (1) for households located in villages assigned to treatment, and equal to (0) for households located in control villages. In order to estimate the LATE, I also created a **treated** measure equal to (1) for households assigned to treatment that actually take the treatment—which means women received all the cash installments in the

observed period—and equal to (0) for households that did not take the treatment.

Second, to examine whether they were differential marginal returns to Progresa according to women's economic dependence, I created a variable measuring couples' **relative income at baseline**. Given that less than 10% of female recipients participated in paid work before the program, the relative income measure is operationalized as a binary indicator that equals 1 if the husband provided all the couples' shared income or 0 if the wife contributed any earnings.

Control Variables

In the context of an experimental design the inclusion of covariates serves the purpose of increasing the precision of estimates (Gerber and Green, 2012). In this case, I included a series of individual characteristics: respondent's ethnicity, years of education, age, labor participation and marital status. In addition, I included all variables that were imbalanced at baseline or were predictive of attrition. These covariates include yearly household income, and whether respondents' own land, own animals, have electricity, own TV, own radio, and own car.

Analytical Strategy

In this study, I estimate two quantities of interest: the intent-to-treat (ITT) and the local average treatment effect (LATE) of Progresa. The ITT refers to the treatment effect of the program on the randomly assigned units, regardless if they actually receive the treatment. To obtain the ITT for all outcomes of interest, including the different indexes of household dynamics, I estimated linear probability models adjusting for pre-treatment covariates. The main specification to estimate the ITT is of the following form:

$$Outcome_{ij} = \beta_0 + \beta_1 Assigned_{ij} + \beta_2 \mathbf{X_i} + u_{ij} \tag{1}$$

Where $Assigned_i j$ is the treatment assignment indicator for household i in village j, $\mathbf{X_i}$, is a

vector of covariates of household i, and u_ij are robust standard errors clustered at the village level, the unit of randomization.

The LATE is the estimate for the treatment effect on the randomized units that received the treatment; in this case, those who at least received one cash transfer. To obtain the LATE, I use an instrumental variable estimator, where random assignment is used as an instrument for receiving Progresa. The instrument must predict treatment, but should fulfill the exclusion restriction assumption: only affecting the outcome through the treatment (Gerber and Green 2012). Given that the instrument is randomly assigned we can assume this assumption holds in this case. This approach involves a two-stage procedure. The first stage accounts for the possibility that taking the treatment is endogenous, or related to unobserved factors. To deal with this source of endogeneity, I instrument the treatment assignment on an indicator for receiving treatment. Where $Assigned_{ij}$ is an indicator for treatment assignment for household i in village j, X_i, is a vector of covariates for household i, and e_{ij} are robust standard errors clustered at the village level.

$$Treated_{ij} = \alpha_0 + \alpha_1 Assigned_{ij} + \alpha_3 \mathbf{X_i} + e_{ij} \tag{2}$$

The second stage is a linear probability model for the outcome of interest. The key parameter is $\beta_1 Treat_{ij}$, which is an indicator of taking the treatment for household i in village j. This term yields the LATE estimate.

$$Outcome_{ij} = \beta_0 + \beta_1 \widehat{Treated}_{ij} + \beta_3 \mathbf{X_i} + u_{ij} \tag{3}$$

Both equations include the same set of covariates, and are estimated simultaneously. To estimate heterogeneous treatment effects by couples' relative income, I further include treatment by covariate interactions with a dummy variable for relative income at baseline. For all main findings, I will present ITT estimates without covariates (relying on pure random assignment), ITT estimates adjusting for covariates, and LATE estimates adjusting for covariates. Comprehensive

models are found in Appendix B (Tables 16 - 26).

Results

This section discusses the main findings pertaining to Progresa's effects on the perceptions of responsibility for housework, intra-household decision making, and female autonomy. As Table 1 shows, at baseline husbands were the sole earners in most couples—less than 10 percent of wives' participated in the labor market—and on average their yearly salary was around 21 times their wives' income. Around 61 percent of husbands were employed as farm labourers, while 11 percent had small agricultural businesses, and 10 percent worked as non-agricultural workers. The few wives in the labor market worked as non-agricultural employees and domestic workers.

A first glance to the effects of cash transfers is provided by their impact on the different indexes of household dynamics. Both ITT and LATE estimates in Table 2 show that Progresa significantly increased female autonomy and reduced the attribution of housework to wives in, at least, 5 and 3 percentage points, respectively. As expected, the magnitude of these effects is larger if we focus only on households that received at least one cash transfer (LATE estimates). Nevertheless, the index of decision-making also reveals that Progresa did not impact this dimension of household dynamics.

[Table 2 here]

We now focus on the effects of Progresa on specific measures of household dynamics. Regarding perceptions of housework, Table 3 shows that across specifications receiving Progresa cash transfers significantly decreased the probability of allocating to mothers the duty of doing laundry. For instance, LATE estimates indicate a reduction in 3 percentage points in the attribution of this task $(p < 0.01)$. This means that while 99% of husbands in the control group attributed doing laundry to their wives, 96% of husbands in treated households allocated this task to their spouses. In addition, point estimates indicate that Progresa also decreased the probability of attributing the

chores of carrying water and taking care of (house) animals solely to wives. Also, the program had an effect close to zero on child care—the only task not considered as housework for exchange theories.

[Table 3 here]

Estimates in Table 4 show that Progresa cash transfers only moderately increased the chores that husbands perceive as their responsibility. In particular, the program had a statistically significant effect on raising the probability that husbands perceive doing the laundry as their responsibility in 1 percentage point ($p < 0.01$). In other words, while only 7.7% of men in the control group perceived doing laundry as their responsibility, 9% of husbands in treated households perceived this was the case. Moreover, all traditionally "female" tasks show positive but statistically insignificant point estimates. Together, the statistically significant reduction in attributing doing laundry to wives, is mirrored by an increase of men's perception of doing laundry as their responsibility, suggesting a "compensatory" dynamic.

[Table 4 here]

Treatment effects on specific areas of decision-making confirm that Progresa had a moderate impact in this dimension of household dynamics. As Table 5 shows, the program only somewhat increased the probability that mothers decide alone about children spending in 2 percentage points ($p < 0.1$). This means that while 41 percent of mothers in the control group freely decided about children expenditures, 43 percent did so in the treatment group. In addition, models that directly measure whether this increase in women's decision making was in detriment of their husbands' power, confirm this is the case for decisions concerning children spending (See Table 11 in the Appendix).

[Table 5 here]

Table 6 shows results for father's decision-making versus everyone else in the home. As shown, most effect sizes are close to zero with the exception of children spending, yet this change was only significant ($p < 0.1$) in the ITT estimates that include covariates. While these results indicate that cash transfers did not considerably shaped decision-making power between spouses, they also show that Progresa did not undermine wives' position in the household, as gender backlash perspectives predicted.

[Table 6 here]

Receiving a Progresa cash transfer did have a positive impact on specific dimensions of female autonomy. As Table 7 shows, the program significantly increased the probability that husbands perceive women should administer their own cash by at least 5 percentage points ($p < 0.05$). In other words, while 33% of husbands in the control group think that women should be able to administer their own cash, almost 39% of respondents agreed with this statement in the treatment group. In addition, Progresa increased by at least 2 percentage points wives' ability to go out alone ($p < 0.1$). However, the program only reduced in 1 percentage point ($p < 0.1$) the idea that husbands should administer their wives' cash and did not have an impact on wives' needing their spouse's permission to visit a friend.

[Table 7 here]

Are there differential marginal returns to Progresa transfers?

The analysis of heterogeneous treatment effects by spouses' relative income serves to directly explore if there are different marginal returns to Progresa transfers according to wives' economic dependence at baseline. Overall, intent-to-treat estimates (ITT) indicate there were no heterogeneous effects of Progresa according to partners' relative income at baseline. However, models that focus on households that actually received cash transfers (LATE estimates) show some evidence

of differential marginal returns to Progresa. As table 8 displays, in households where husbands contribute the totality of the couple's income, the treatment significantly increases the probability that men attribute doing laundry as their responsibility in 2 percentage points ($p < 0.05$). This when compared to households where wives were less dependent. No significant results were found for women's attributions for housework (see Table 12 in the Appendix).

[Table 8 here]

In the case of household decision-making, interactions terms reveal no major differences in the returns to Progresa. The only exception is the increase of women's power to take their children to the doctor in 3 percentage points ($p < 0.1$) (see Table 13 in the Appendix). And in the case of fathers, a reduction of their decision making pertaining house repairs in 6 percentage points ($p < 0.1$) and children spending in 5 percentage points ($p < 0.1$) (see in Table 14 in the Appendix). Finally, no important differences across groups were found for women's autonomy (see Table 15 in the Appendix).

In sum, this analysis shows they are no major differences in the returns to Progresa cash transfers according to wives' level of economic dependence. The only exception is in the area of housework attitudes—doing laundry specifically—where economically dependent women potentially benefit more from the program, and likewise, their husbands compensatory response, is higher than in the comparison group. This was confirmed by F-tests that examine the significance of both main and interaction effects in each model. Its important to note that previous research has found decreasing marginal returns to women's earnings only in the case of housework (Hook 2017; Killewald and Gough 2010), which is consistent with these findings.

Discussion and Concluding Remarks

Sociological studies from around the globe have examined the relationship between women's earnings and household dynamics, such as housework and decision-making (Bittman et al. 2003; Ev-

ertsson and Nermo 2004; Gupta 2007; Weitzman 2014b). This study enhanced the existing literature by focusing on the effects of welfare money, a non-trivial source of income for many low-SES women in a myriad of countries, on these outcomes (United Nations, 2009). In particular, I examined whether welfare money changes women's position in the household by studying the case of Progresa in Mexico, a conditional cash transfer program first implemented following an experimental design.

Findings indicate that Progresa had a positive effect, circumscribed to attitudinal measures of housework and female autonomy. In particular, the program significantly decreased the probability of husbands allocating to wives the duty of doing laundry. Autonomy measures also indicate that Progresa positively changed husbands' views about women administering their own cash. And for wives that were more economically dependent of their spouses at baseline, receiving a cash transfer had greater marginal returns in terms of their husbands' housework attitudes. Meanwhile, cash transfers did not impact wives' nor husbands' decision-making.

The positive effects of this cash transfer program on household dynamics, which oscillate between 1 to 6 percentage points, are similar to what other studies of Progresa have found in areas like health and education. For instance, after 1.5 years Progresa increased secondary enrollments in 6 percentage points for boys and 9 percentage points for girls (Schultz 2004), reduced child labor in 1.3 percentage points (Skoufias et al. 2001), and improved older adults' labor participation in 4.7 percentage points (Behrman and Parker 2013). Scholars and policymakers considered these effect sizes as substantial, hence the later adoption of this policy design in myriad of developing countries (Parker and Todd 2017).

The findings of this study are consistent with both resource-based—including autonomy and resource-exchange theories—and time constraints perspectives. As the autonomy hypothesis posits, wives receiving Progresa payments did somewhat improve their position in terms of their husbands attitudes towards housework and female autonomy. And in the case of doing laundry this analysis revealed the existence of resource-exchange dynamics. Indeed, while the sole

allocation of doing the laundry to wives decreased, it significantly increased for husbands; showing evidence of a compensatory response. However, these findings, especially those pertaining to housework attitudes, are also consistent with the time-constraint hypothesis. Given that Progresa welfare payments require women to spend more time outside the home fulfilling conditions, it is possible that husbands shifted their perceptions of housework responsibilities to make up for these time constraints. This study cannot disentangle which of these two perspectives explain the results found for Progresa transfers. And in fact, previous literature has found these approaches to be complementary in a handful of empirical cases (Bianchi et al. 2000; Knudsen and Waerness 2007; Hook 2017).

Beyond these hypotheses, I argue that two features of Progresa's design were especially relevant for observing these results—the fact that mothers had to fulfill monthly conditions to receive the cash transfers, and the State's framing of these payments as money mothers *deserved* in recognition for their care work. These two features infused Progresa cash transfers with notions of effort and deservingness that warranted a compensatory response from husbands. This point is especially relevant when we think about welfare programs. As Zelizer (1989) and others have shown (Sykes et al. 2015), this kind of money can sometimes be perceived as a charity, while in other contexts, it might be seen as a fair reward, depending on factors like the program's design and its recipients. Thus, the perception that women "earned" or deserved their Progresa transfers was probably pivotal to improving their position in the home.

In addition, this study contributed to the ongoing debate on targeted programs promoting women's empowerment in developing countries. Initiatives such as conditional cash transfers and micro-credits seek to reduce social and gender inequalities by improving women's material conditions, but whether they actually enhance women's status remains an open question (Moser 1989; Pierotti et al. 2018). In particular, the provision of CCTs solely to mothers—a feature replicated by CCT programs in several countries—has been a source of dispute. Proponents argue this design enhances female empowerment, while critics sustain it reinforces the traditional division of labor (Molyneux 2006; Nagels 2016). This analysis shows Progresa did not generate a gender back-

lash response and recipients were not pushed further into unpaid labor, as gender-based theories predicted.

But apart from gender-backlash responses, various scholars have also been skeptic about the potential of targeted programs to reduce gender inequality. Although I found substantial effects of cash transfers on female autonomy and some dimensions of housework, the program had a modest impact on women's decision-making. As some of these scholars have posited, the latter could be a result of the persistence of traditional gender ideals in rural Mexico (Molyneux 2006; Nagels 2016). Indeed, the relevance of gender ideology at the individual and national level for intra-household gender inequality has been extensively documented in the family literature (Cunningham 2007; Hook 2006). For instance, cross-country studies have shown that women in less-egalitarian contexts benefit less from their individual earnings in areas like the division of household labor and decision-making (Knudsen and Waerness 2007). Thus, the prevalence of traditional gender ideals in contexts like rural Mexico (Romero Rabell 2009) probably undermines the potential of targeted policies like welfare payments to transform gender relations in more significant ways.

Lastly, is important to remember the limitations of the outcome measures of this work. In particular, measures for housework attitudes, female autonomy and decision-making traced only absolute changes between spouses—who should be in charge of laundry, for example. These discrete outcomes might be concealing higher marginal changes that sensitive measures, such as time use, would be able to capture.

Overall, this study illustrates how can we exploit field experiments to answer sociological questions concerning the family, a strategy that could be increasingly fruitful given the prominence of randomized control trials in the policy arena. In this article, I find that conditional cash transfers effectively improved women's position in the household only after 1.5 years of its implementation, and is possible that the magnitude of this effect had grown over time. In order to fully assess the effectiveness of conditional cash transfers for reducing within-household gender inequalities,

future research should explore alternative strategies to analyze the effects of these programs in the long run.

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Tables

Table 1: ***Pretreatment Covariate Balance***

	Treatment		**Control**		
	Mean	N	Mean	N	p-value
Household Demographics					
Number of children	3.88	5817	3.83	3438	0.871
Yearly Household Income	11332.67	6139	11489.12	3612	0
Own animals	0.36	6134	0.33	3608	0.002
Own land	0.62	6124	0.59	3609	0.029
Own house	0.94	5756	0.91	3343	0
House Characteristics					
Bathroom	0.56	6129	0.55	3601	0.383
Electricity	0.61	6135	0.63	3610	0.073
Running water	0.15	1898	0.16	876	0.615
Goods					
Refrigerator	0.96	6135	0.96	3610	0.188
Gas heater	0.84	6136	0.84	3612	0.709
Gas stove	0.98	6122	0.98	3604	0.726
Radio	0.46	6130	0.44	3612	0.116
TV	0.69	6135	0.66	3610	0
Washing machine	0.99	6133	0.99	3611	0.357
Car	0.99	6134	1	3612	0.027
Truck	0.98	6133	0.98	3612	0.939
Characteristics Wives					
Age	36.64	6069	36.74	3578	1

Indigenous Heritage	0.42	6070	0.44	3574	0.16
Years of Education	2.62	6066	2.59	3574	0.476
Labor Participation	0.1	6057	0.07	3568	0
Yearly Income	287.33	6040	226.51	3563	0.223
Married	0.75	6080	0.7	3581	0
Cohabitation	0.25	6080	0.3	3581	0
Characteristics Husbands					
Age	41.05	6128	41.44	3600	0.222
Indigenous Heritage	0.42	6140	0.45	3600	0.119
Years of Education	2.96	6125	2.84	3600	0.138
Labor Participation	0.94	6061	0.94	3557	0.207
Yearly Income	6271	5992	6830.2	3562	0
Married	0.75	6148	0.7	3607	0
Cohabitation	0.25	6148	0.3	3607	0

Table 2: ***Treatment Effects Indexes of Household Dynamics***

	Index HA	Index Decision	Index Autonomy
ITT	-0.03*	0.02	0.05**
	(0.02)	(0.02)	(0.02)
ITT + Cov	-0.04**	0.02	0.05**
	(0.02)	(0.02)	(0.02)
LATE + Cov	-0.05**	0.03	0.07**
	(0.03)	(0.03)	(0.03)
ITT N	9751	9742	9748
ITT Attrition Bounds	[-0.07, -0.01]	[-0.01, 0.14]	[0.01, 0.06]
Note: Clustered standard errors in parentheses;*p<0.1; **p<0.05; ***p<0.01			

Requires LaTeX packages: dcolumn

Table 3: ***Treatment Effects Attribution of Housework to Wives***

	Dependent variable:			
	Do laundry	Carry water	Take care of animals	Childcare
ITT	$-0.02**$	$-0.03**$	$-0.03*$	0.00
	(0.01)	(0.01)	(0.02)	(0.01)
ITT + Cov	$-0.02**$	$-0.02**$	$-0.03*$	-0.01
	(0.01)	(0.01)	(0.02)	(0.01)
LATE + Cov	$-0.03**$	-0.03	$-0.04*$	-0.01
	(0.01)	(0.03)	(0.02)	(0.02)
ITT N	9751	9740	9707	9733
Attrition Bounds	$[-0.02, -0.01]$	$[-0.04, -0.03]$	$[-0.04, -0.03]$	$[-0.01, 0.00]$
Observations	8514	8505	8474	8499
Note:				*p<0.1; **p<0.05; ***p<0.01

Table 4: ***Treatment Effects Attribution of Housework to Husbands***

	Dependent variable:			
	Do laundry	Carry water	Take care of animals	Childcare
ITT	$0.01**$	0.02	0.01	0
	(0.00)	(0.01)	(0.01)	(0.01)
ITT + Cov	$0.01**$	0.01	0.01	0.005
	(0.00)	(0.01)	(0.01)	(0.01)
LATE + Cov	$0.01**$	0.02	0.01	0.01
	(0.00)	(0.02)	(0.02)	(0.01)
ITT N	9751	9740	9707	9733
ITT Attrition Bounds	$[0.00, 0.01]$	$[0.01, 0.02]$	$[0.00, 0.01]$	$[-0.01, 0.00]$
Note:				*p<0.1; **p<0.05; ***p<0.01

Table 5: ***Treatment Effects Wives' Decision Making***

	Dependent variable:			
	Children school	Children doctor	House repairs	Children spending
ITT	0.01	0.00	0.00	$0.02*$
	(0.01)	(0.02)	(0.01)	(0.01)
ITT + Cov	0.00	0.00	0.01	$0.02*$
	(0.01)	(0.01)	(0.01)	(0.01)
LATE + Cov	0.00	0.00	0.01	$0.02*$
	(0.01)	(0.01)	(0.01)	(0.01)
ITT N	9656	9684	9726	9684
ITT Attrition Bounds	$[0.00, 0.01]$	$[0.00, 0.01]$	$[-0.01, 0.01]$	$[0.01, 0.02]$
Note:				*p<0.1; **p<0.05; ***p<0.01

Table 6: ***Treatment Effects Husbands' Decision Making***

	Dependent variable:			
	Children school	Children doctor	House repairs	Children spending
ITT	0.00	0.00	0.00	−0.02
	(0.01)	(0.01)	(0.02)	(0.01)
ITT + Cov	0.00	0.00	−0.01	−0.02∗
	(0.01)	(0.01)	(0.02)	(0.01)
LATE + Cov	0.00	0.01	0.01	0.03
	(0.01)	(0.01)	(0.02)	(0.02)
ITT N	9656	9684	9726	9684
ITT Attrition Bounds	[− 0.01, 0.00]	[− 0.01, 0.00]	[− 0.01, 0.00]	[− 0.03, −0.02]
Note:				∗p<0.1; ∗∗p<0.05; ∗∗∗p<0.01

Table 7: ***Treatment Effects Female Autonomy***

	Dependent variable:			
	Women control cash	Go out alone	Partner's permission	Husband women's cash
ITT	0.05 ∗ ∗	0.02 ∗ 0	−0.01∗	
	(0.02)	(0.01)	(0.01)	(0.01)
ITT + Cov	0.05 ∗ ∗	0.02∗	−0.001	−0.01∗
	(0.02)	(0.01)	(0.01)	(0.01)
LATE + Cov	0.06 ∗ ∗	0.03∗	0.00	0.01∗
	(0.03)	(0.02)	(0.01)	(0.01)
ITT N	9746	9741	9481	9741
ITT Attrition Bounds	[0.04, 0.05]	[0.01, 0.03]	[− 0.01, 0.00]	[− 0.02, −0.01]
Note:				∗p<0.1; ∗∗p<0.05; ∗∗∗p<0.01

Table 8: ***Heterogeneous Treatment Effects: Attribution of Housework to Husbands***

	Dependent variable:			
	Do laundry	Carry water	Take care of animals	Childcare
Treatment	−0.011	−0.001	0.012	−0.003
	(0.007)	(0.025)	(0.028)	(0.013)
Relative Income 97	−0.002	−0.014	0.022	0.0005
	(0.006)	(0.021)	(0.021)	(0.008)
Treatment x Relative Income 97	0.018∗∗	−0.0004	−0.004	0.002
	(0.008)	(0.026)	(0.031)	(0.013)
Constant	0.056	0.380∗∗∗	0.186∗∗	0.116∗∗
	(0.037)	(0.098)	(0.093)	(0.048)
Controls	Yes	Yes	Yes	Yes
Observations	8345	8336	8307	8330
F-test (Rel + TreatxRel = 0)	6.346	0.472	1.061	0.024
p-value	0.002	0.624	0.346	0.977
Note:				∗p<0.1; ∗∗p<0.05; ∗∗∗p<0.01

Appendix A

Table 9: Descriptive Statistics Attriters vs. Not Attriters

	Attriters		**Not attriters**	
	Mean	N	Mean	N
Household Demographics				
Number of children	3.57	1106	3.86	9255
Yearly Household Income	11912.24	1203	11390.62	9751
Own animals	0.29	1202	0.35	9742
Own land	0.51	1203	0.61	9733
Own house	0.91	1052	0.93	9099
House Characteristics				
Bathroom	0.38	1203	0.55	9730
Electricity	0.56	1203	0.62	9745
Running water	0.13	311	0.15	2774
Goods				
Refrigerator	0.95	1203	0.96	9745
Gas heater	0.79	1203	0.84	9748
Gas stove	0.98	1203	1	9726
Radio	0.51	1203	0.46	9742
TV	0.71	1203	0.69	9745
Washing machine	0.99	1203	0.99	9744
Car	1	1203	1	9746
Truck	0.98	1203	0.99	9745
Characteristics Wives				
Age	34.56	1189	36.67	9647
Indigenous Heritage	0.35	1194	0.43	9644
Years of Education	2.71	1193	2.61	9640
Labor Participation	0.15	1193	0.09	9625
Yearly Income	456.79	1186	264.77	9603
Married	0.74	1195	0.74	9661

Cohabitation	0.26	1195	0.27	9661
Characteristics Husbands				
Age	38.76	1198	41.2	9728
Indigenous Heritage	0.37	1201	0.44	9740
Years of Education	2.85	1200	2.92	9725
Labor Participation	0.94	1186	0.94	9618
Yearly Income	7794.05	1161	6479.49	9554
Married	0.74	1202	0.74	9751
Cohabitation	0.26	1202	0.27	9751

Table 10: OLS Estimates Probability of Attrition

	Coefficient	Standard Errors	P value
Treatment	0.11	0.14	0.44
Household Characteristics			
Number of children			
Number of children x treatment	0	0	0.38
Yearly household income	0	0	0.14
Yearly household income x treatment	0	0	0.17
Own animals	-0.01	0.01	0.37
Own animals x treatment	0	0.02	0.84
Own land	-0.03	0.03	0.18
Own land x treatment	0.04	0.03	0.13
Own house	0.05*	0.02	0.04
Own house x treatment	-0.09*	0.04	0.05
Bathroom	-0.06***	0.02	0
Bathroom x treatment	0.01	0.02	0.6
Electricity	0.02	0.02	0.38
Electricity x treatment	-0.06**	0.02	0.03
Own fridge	-0.02	0.03	0.39

Own fridge x treatment	0	0.03	0.98
Gas heater	-0.06	0.03	0.07
Gas heater x treatment	0.03	0.04	0.37
Gas stove	0.02	0.04	0.65
Gas stove x treatment	-0.05	0.04	0.18
Radio	0.02*	0.01	0.05
Radio x treatment	-0.02	0.02	0.26
TV	0	0.02	0.81
TV x treatment	0	0.02	0.93
Washing machine	0.04	0.06	0.52
Washing machine x treatment	-0.05	0.07	0.44
Car	-0.04	0.07	0.57
Car x treatment	-0.01	0.1	0.94
Truck	-0.06	0.09	0.48
Truck x treatment	0.07	0.09	0.47
Respondent's Characteristics			
Age			
Age 26-30	-0.03	0.01	0.07
Age 31-35	-0.05***	0.01	0
Ages 35-41	-0.07***	0.02	0
Ages 42-50	-0.07***	0.02	0
Ages 51+	-0.09***	0.02	0
Age x treatment			
Age 26-30 x treatment	0	0.02	0.94
Age 31-35 x treatment	0	0.02	0.94
Ages 35-41 x treatment	0.01	0.03	0.71
Ages 42-50 x treatment	0.03	0.03	0.33
Ages 51+ x treatment	0.03	0.03	0.43
Indigenous Heritage	-0.02	0.03	0.51
Indigenous Heritage x treatment	0	0.03	1
Years of Education	0	0	0.17
Years of Education x treatment	0	0	0.74
Married	0.03*	0.01	0.04

Married x treatment	-0.02	0.02	0.24
Constant	0.26	0.1	0.01
N	18,523		
* p<0.05, ** p<0.01, *** p<0.001			

Requires LaTeX packages: dcolumn

Table 11: ***Treatment Effects Husbands to Wives' Decision Making, Alternative Coding***

	Dependent variable:			
	Do laundry	Carry water	Take care of animals	Childcare
ITT	0.02	0.01	0.00	0.05*
	(0.04)	(0.04)	(0.01)	(0.03)
ITT + Cov	0.01	0.01	0.00	0.05*
	(0.04)	(0.04)	(0.01)	(0.03)
LATE + Cov	−0.01	0.00	0.01	0.06
	(0.05)	(0.05)	(0.02)	(0.04)
ITT N	2017	1934	3375	2545
Note:				*p<0.1; **p<0.05; ***p<0.01

Table 12: ***Heterogeneous Treatment Effects: Attribution of Housework to Wives***

	Dependent variable:			
	Do laundry	Carry water	Take care of animals	Childcare
Treatment	0.02	0.01	−0.02	−0.01
	(0.019)	(0.032)	(0.028)	(0.027)
Relative Income 97	0.02	0.05**	0.05**	−0.03
	(0.017)	(0.025)	(0.025)	(0.019)
Treatment x Relative Income 97	−0.03	−0.020	0.00	0.01
	(0.020)	(0.035)	(0.030)	(0.027)
Constant	0.998***	0.507***	0.430***	0.929***
	(0.066)	(0.130)	(0.099)	(0.077)
Controls	Yes	Yes	Yes	Yes
Observations	8345	8336	8307	8330
F-test (Rel + TreatxRel = 0)	0.956	2.569	3.566	1.254
p-value	0.384	0.077	0.028	0.285
Note:				*p<0.1; **p<0.05; ***p<0.01

Table 13: ***Heterogeneous Treatment Effects: Wives' Decision Making***

	Dependent variable:			
	Children school	Children doctor	House repairs	Children spending
Treatment	-0.035	-0.030^{**}	-0.033^{*}	-0.010
	(0.027)	(0.014)	(0.018)	(0.017)
Relative Income 97	-0.040^{**}	-0.034^{***}	-0.033^{***}	-0.042^{***}
	(0.016)	(0.013)	(0.012)	(0.013)
Treatment x Relative Income 97	0.031	0.029^{*}	0.027	0.016
	(0.029)	(0.016)	(0.020)	(0.018)
Constant	0.022	0.497^{***}	0.414^{***}	0.467^{***}
	(0.055)	(0.031)	(0.052)	(0.044)
Observations	8269	8290	8323	8293
F-test (Rel + TreatxRel = 0)	3.297	3.721	3.89	7.053
p-value	0.037	0.024	0.02	0.001
Note:				*p<0.1; **p<0.05; ***p<0.01

Table 14: ***Heterogeneous Treatment Effects: Husbands' Decision Making***

	Dependent variable:			
	Children school	Children doctor	House repairs	Children spending
Treatment	0.009	0.029	0.064^{**}	0.033
	(0.019)	(0.020)	(0.032)	(0.026)
Relative Income 97	0.013	0.026^{*}	0.054^{***}	0.054^{***}
	(0.013)	(0.014)	(0.019)	(0.019)
Treatment x Relative Income 97	-0.013	-0.026	-0.061^{*}	-0.049^{*}
	(0.020)	(0.021)	(0.034)	(0.027)
Constant	0.061	0.035	0.186^{*}	0.123
	(0.053)	(0.063)	(0.096)	(0.083)
Controls	Yes	Yes	Yes	Yes
Observations	8269	8290	8323	8293
F-test (Rel + TreatxRel = 0)	0.483	1.847	4.078	4.265
p-value	0.617	0.158	0.017	0.014
Note:				*p<0.1; **p<0.05; ***p<0.01

Table 15: ***Heterogeneous Treatment Effects: Female Autonomy***

	Dependent variable:			
	Women's cash	Go out alone	Partner's Permission	Husband's women's cash
Treatment	0.002	-0.014	-0.024	0.002
	(0.033)	(0.029)	(0.016)	(0.011)
Relative Income 97	-0.043	-0.084^{***}	-0.009	0.005
	(0.027)	(0.022)	(0.013)	(0.006)
Treatment x Relative Income 97	0.037	0.021	0.020	-0.010
	(0.031)	(0.028)	(0.018)	(0.010)
Constant	0.382^{***}	0.206^{***}	0.026	0.014
	(0.110)	(0.079)	(0.041)	(0.023)
Controls	Yes	Yes	Yes	Yes
Observations	8342	8338	8119	8342
F-test (Rel + TreatxRel = 0)	1.265	9.891	37.322	0.5
p-value	0.282	0	0	0.606
Note:				*p<0.1; **p<0.05; ***p<0.01

Appendix B

Full ITT and LATE Models with Covariates

Table 16: ***Treatment Effects on Index Outcomes***

	ITT + Covariates			LATE + Covariates		
	Housework	Decision Making	Autonomy	Housework	Decision Making	Autonomy
Treatment	-0.04**	0.02	0.05**	-0.050**	0.03	0.073**
	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)
Indigenous Heritage	0.05***	-0.070***	-0.05**	0.046***	-0.070***	-0.050**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Sex	0.04	0.16	0.13	0.04	0.16	0.14
	(0.08)	(0.13)	(0.1)	(0.08)	(0.13)	(0.1)
Years of Education	0.00	0.01*	-0.00	0.00	0.01	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Age	0.00***	0.00**	0.00***	0.00***	0.00**	0.00***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Labor Participation	-0.04	-0.14***	-0.02	-0.04	-0.15***	-0.02
	(0.03)	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)
Own land	0.01	-0.04**	-0.01	0.01	-0.04**	-0.01
	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)
Own animals	-0.04***	-0.02	-0.03	-0.04***	-0.02	-0.03
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Electricity	-0.00	0.01	-0.07***	0.00	0.01	-0.07***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Own TV	0.00	-0.02	-0.00	0.00	-0.02	0.00
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Married	-0.08***	0.00	0.04**	-0.08***	0.00	0.04**
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Household Income	0.00	0.02**	-0.01	0.00	0.02**	-0.01*
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Own radio	0.02	0.01	0.00	0.02	0.01	0.00

	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)
Own car	-0.01	0.03	0.06	-0.01	0.03	0.06
	(0.07)	(0.06)	(0.08)	(0.08)	(0.06)	(0.08)
Own house	-0.01	0.04	-0.01	-0.01	0.04	-0.01
	(0.03)	(0.04)	(0.03)	(0.03)	(0.04)	(0.03)
Constant	0.27***	-0.17	-0.07	0.27***	-0.17	-0.07
	(0.1)	(0.11)	(0.12)	(0.10)	(0.11)	(0.12)
N	8514	8507	8512	8514	8507	8512

Note: Clustered standard errors in parentheses p<0.1; p<0.05; p<0.01

Table 17: ***ITT Effects Attribution of Housework to Wives***

	Dependent variable:			
	Do laundry	Carry water	Take care of animals	Childcare
Treatment	-0.02***	-0.02*	-0.03*	-0.01
	(0.01)	(0.01)	(0.02)	(0.01)
Indigenous	-0.01	0.05***	0.06***	0.00
	(0.01)	(0.02)	(0.02)	(0.01)
Sex	0.05	-0.08***	-0.06	0.07
	(0.03)	(0.01)	(0.08)	(0.05)
Years of Education	0.003**	0.001	0.002	0.00
	(0.0)	(0.0)	(0.0)	(0.0)
Age	-0.00***	-0.00	-0.00***	-0.00***
	(0.0)	(0.0)	(0.0)	(0.0)
Labor Participation	0.00	-0.05	-0.04*	0.02
	(0.01)	(0.02)	(0.02)	(0.02)
Own land	0.01	0.02	-0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)
Own animals	-0.01	-0.04	-0.04***	0.003
	(0.01)	(0.01)	(0.01)	(0.01)
Electricity	0.00	0.01	0.00	0.01

	(0.01)	(0.02)	(0.02)	0.01
Own TV	0.01	-0.01	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.01)
Married	-0.02***	-0.09	-0.07***	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)
Household Income	0.00	0.00	0.00	0.00
	(0.0)	(0.0)	(0.01)	(0.01)
Own radio	0.00	0.03	0.00	0.01
	(0.01)	(0.01)	(0.01)	(0.01)
Own car	0.00	0.04	0.01	-0.07
	(0.05)	(0.09)	(0.07)	(0.04)
Own house	-0.01	-0.03	-0.03	0.01
	(0.01)	(0.02)	(0.03)	(0.02)
Constant	0.99***	0.58***	0.50***	0.91***
	(0.07)	(0.01)	(0.09)	(0.07)
Observations	8514	8505	8474	8499

Note: Clustered standard errors in parentheses p<0.1; p<0.05; p<0.01

Table 18: ***ITT Effects Attribution of Housework to Husbands***

	Dependent variable:			
	Do laundry	Carry water	Take care of animals	Childcare
Treatment	0.01***	0.01	0.01	0.01
	(0.00)	(0.01)	(0.01)	(0.01)
Indigenous Heritage	0.00	-0.02	-0.01	0.00
	(0.00)	(0.02)	(0.01)	(0.01)
Sex	0.00	0.12***	0.03	-0.02
	(0.02)	(0.01)	(0.06)	(0.02)
Years of Education	0.00	0.00	0.00	0.00
	(0.0)	(0.0)	(0.0)	(0.0)
Age	0.00	0.00	0.00	0.00
	(0.0)	(0.0)	(0.0)	(0.0)
Labor Participation	-0.01	0.02	0.02	0.00
	(0.01)	(0.02)	(0.02)	(0.01)
Own land	0.00	-0.02	0.04***	0.00
	(0.00)	(0.01)	(0.01)	(0.01)
Own animals	0.00	0.00	0.01	-0.01***
	(0.00)	(0.01)	(0.01)	(0.00)
Electricity	-0.01*	-0.04	-0.02*	-0.01
	(0.00)	(0.01)	(0.01)	(0.01)
Own TV	0.00	0.02	0.01	0.00
	(0.01)	(0.01)	(0.01)	(0.01)
Married	0.00	0.05	0.00	0.00
	(0.00)	(0.01)	(0.01)	(0.01)
Household Income	0.00	-0.01	0.00	0.00
	(0.00)	(0.01)	(0.01)	(0.00)
Own radio	0.00	0.00	0.03***	0.00
	(0.00)	(0.01)	(0.01)	(0.01)
Own car	-0.04	-0.11	0.02	-0.01
	(0.03)	(0.07)	(0.07)	(0.03)
Own house	0.00	-0.01	0.00	-0.01

	(0.01)	(0.02)	(0.02)	(0.01)
Constant	0.08*	0.35***	0.20**	0.12***
	(0.04)	(0.01)	(0.09)	(0.04)
Observations	8514	8505	8474	8499
Note: Clustered standard errors in parentheses p<0.1; p<0.05; p<0.01				

Table 19: ***ITT Effects Wives' Decision Making***

	Dependent variable:			
	Children school	Children doctor	House repairs	Children spending
Treatment	0.00	0.00	0.01	0.02*
	(0.01)	(0.01)	(0.01)	(0.01)
Indigenous	-0.01	-0.02**	-0.02***	-0.03***
	(0.01)	(0.01)	(0.01)	(0.01)
Sex	0.08	0.05***	0.03	0.03
	(0.06)	(0.01)	(0.04)	(0.04)
Years of Education	0.00	0.00	0.00	0.00
	(0.0)	(0.0)	(0.0)	(0.0)
Age	0.00**	0.00	0.00	0.00*
	(0.0)	(0.0)	(0.0)	(0.0)
Labor Participation	-0.02	-0.03	-0.04***	-0.06***
	(0.02)	(0.01)	(0.01)	(0.01)
Own land	-0.01	-0.01	-0.02**	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)
Own animals	-0.02***	-0.01	0.01	0.00
	(0.01)	(0.01)	(0.01)	(0.01)
Electricity	-0.01	0.00	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)
Own TV	-0.01	-0.01	-0.01	0.00
	(0.01)	(0.01)	(0.01)	(0.01)
Married	0.00	0.00	0.00	0.00

	(0.01)	(0.01)	(0.01)	(0.01)
Household Income	0.01***	0.00	0.00	0.01**
	(0.0)	(0.0)	(0.0)	(0.0)
Own radio	0.01	0.00	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.01)
Own car	0.02	0.03	-0.01	-0.02
	(0.03)	(0.03)	(0.03)	(0.02)
Own house	0.02	0.01	0.01	0.00
	(0.01)	(0.01)	(0.01)	(0.01)
Constant	-0.04	0.45***	0.37***	0.41***
	(0.05)	(0.01)	(0.05)	(0.04)
N	8435	8458	8492	8461

Note: Clustered standard errors in parentheses p<0.1; p<0.05; p<0.01

Table 20: ***ITT Effects Husbands' Decision Making***

	Dependent variable:			
	Children school	Children doctor	House repairs	Children spending
Treatment	0.00	0.00	-0.01	-0.02*
	(0.01)	(0.01)	(0.02)	(0.01)
Indigenous	0.03***	0.03**	0.04**	0.05***
	(0.01)	(0.02)	(0.02)	(0.01)
Sex	0.00	0.00	0.04	0.02
	(0.05)	(0.01)	(0.06)	(0.06)
Years of Education	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Age	0.00	0.00	0.00	0.00
	(0.0)	(0.0)	(0.0)	(0.0)
Labor Participation	0.02	0.02	0.04**	0.06***
	(0.01)	(0.01)	(0.02)	(0.02)
Own land	0.00	0.00	0.02	0.00
	(0.01)	(0.01)	(0.01)	(0.01)
Own animals	0.00	0.00	-0.02	-0.01

	(0.01)	(0.01)	(0.01)	(0.01)
Electricity	-0.01	-0.01	-0.02	-0.02*
	(0.01)	(0.01)	(0.02)	(0.01)
Own TV	0.01	0.01	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)
Married	0.00	0.00	-0.01	-0.01
	(0.01)	(0.01)	(0.02)	(0.01)
Household Income	0.00	0.00	0.00	-0.01*
	(0.00)	(0.00)	(0.01)	(0.01)
Own radio	0.00	0.00	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.01)
Own car	0.00	0.00	0.05	0.07
	(0.05)	(0.05)	(0.07)	(0.05)
Own house	0.00	0.00	-0.02	0.00
	(0.01)	(0.01)	(0.02)	(0.02)
Constant	0.07	0.07***	0.25**	0.19**
	(0.06)	(0.01)	(0.10)	(0.08)
	8435	8458	8492	8461

Note: Clustered standard errors in parentheses p<0.1; p<0.05; p<0.01

Table 21: ***ITT Effects Female Autonomy***

	Dependent variable:			
	Women's cash	Go out alone	Partner's Permission	Husband Women's cash
Treatment	0.05**	0.02*	0.00	-0.01*
	(0.02)	(0.01)	(0.01)	(0.01)
Indigenous	-0.07***	0.001	0.002	0.00
	0.02	(0.01)	(0.01)	0.00
Sex	0.06	0.00	0.07	-0.02***
	(0.07)	(0.05)	(0.05)	(0.01)
Years of Education	0.00	0.00	0.00	-0.00**
	(0.00)	(0.00)	(0.00)	(0.00)
Age	0.00	0.00	0.00	-0.00**

	(0.00)	(0.00)	(0.00)	(0.00)
Labor Participation	0.01	-0.01	-0.01	0.00
	(0.02)	(0.01)	(0.01)	(0.01)
Own land	-0.01	-0.01	0.00	-0.01
	(0.01)	(0.01)	(0.01)	(0.00)
Own animals	-0.01	-0.02	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.00)
Electricity	-0.03	-0.03	-0.02**	0.00
	(0.02)	(0.01)	(0.01)	(0.01)
Own TV	0.01	0.00	-0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)
Married	0.02	0.02	0.00	0.01***
	(0.01)	(0.01)	(0.01)	(0.00)
Household Income	0.00	-0.01	0.00	0.00
	(0.01)	(0.00)	(0.00)	(0.00)
Own radio	-0.01	0.01	0.00	0.01
	(0.01)	(0.01)	(0.01)	(0.00)
Own car	0.06	0.00	0.02	0.01***
	(0.07)	(0.05)	(0.02)	(0.00)
Own house	0.01	-0.01	0.00	0.01
	(0.03)	(0.01)	(0.01)	(0.01)
Constant	0.32***	0.11***	0.01	0.02
	(0.10)	(0.02)	(0.04)	(0.02)
Observations	8511	8507	8285	8511

Note: Clustered standard errors in parentheses p<0.1; p<0.05; p<0.01

Table 22: ***LATE: Attribution of Housework to Wives***

	Dependent variable:			
	Do laundry	Carry water	Take care of animals	Childcare
Treatment	-0.03***	-0.03	-0.04*	-0.01
	(0.01)	(0.03)	(0.02)	(0.02)
Indigenous Heritage	0.00	0.05***	0.06***	0.00

	(0.01)	(0.02)	(0.02)	(0.01)
Sex	0.04	-0.08	-0.06	0.06
	(0.03)	(0.07)	(0.08)	(0.05)
Years of education	0.00***	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Age	0.00***	0.00***	0.00***	0.00***
	(0.00)	(0.00)	(0.00)	(0.00)
Labor Participation	0.00	-0.05**	-0.04*	0.02
	(0.01)	(0.02)	(0.02)	(0.02)
Married	-0.02***	-0.09***	-0.07***	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)
Yearly household income	0.00	0.00	0.00	0.00
	(0.00)	(0.01)	(0.01)	(0.01)
Own land	0.01	0.02	-0.01	0.01
	(0.01)	(0.02)	(0.01)	(0.01)
Own animals	-0.01	-0.04**	-0.04***	0.00
	(0.01)	(0.01)	(0.01)	(0.01)
Have electricity	0.00	0.01	0.00	0.01
	(0.01)	(0.02)	(0.02)	(0.01)
Own TV	0.01	-0.01	0.00	0.00
	(0.01)	(0.02)	(0.01)	(0.01)
Own radio	0.00	0.03***	0.00	0.01
	(0.01)	(0.01)	(0.01)	(0.01)
Own car	0.00	0.04	0.00	-0.07
	(0.05)	(0.10)	(0.07)	(0.04)
Own house	-0.01	-0.03	-0.03	0.01
	(0.01)	(0.02)	(0.03)	(0.02)
Constant	0.99***	0.58***	0.50***	0.91***
	(0.07)	(0.12)	(0.09)	(0.07)
Observations	8,514	8,505	8,474	8,499

Note: Clustered standard errors in parentheses p<0.1; p<0.05; p<0.01

Table 23: ***LATE: Attribution of Housework to Husbands***

	Dependent variable:			
	Do laundry	Carry water	Take care of animals	Childcare
Treatment	0.01***	0.02	0.01	0.01
	(0.00)	(0.02)	(0.02)	(0.01)
Indigenous Heritage	0.00	-0.02	-0.01	0.00
	(0.00)	(0.01)	(0.01)	(0.01)
Sex	0.00	0.12*	0.03	-0.02
	(0.02)	(0.07)	(0.06)	(0.02)
Years of education	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Age	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Labor Participation	-0.01	0.02	0.01	0.00
	(0.01)	(0.02)	(0.02)	(0.01)
Married	0.00	0.05***	0.00	0.00
	(0.00)	(0.01)	(0.01)	(0.01)
Yearly household income	0.00	-0.01**	0.00	0.00
	(0.00)	(0.01)	(0.01)	(0.00)
Own land	0.00	-0.02**	0.04***	0.00
	(0.00)	(0.01)	(0.01)	(0.01)
Own animals	0.00	0.00	0.01	-0.01***
	(0.00)	(0.01)	(0.01)	(0.00)
Have electricity	-0.01*	-0.04***	-0.02*	-0.01
	(0.00)	(0.01)	(0.01)	(0.01)
Own TV	0.00	0.02**	0.01	0.00
	(0.00)	(0.01)	(0.01)	(0.01)
Own radio	0.00	0.00	0.03***	0.00
	(0.00)	(0.01)	(0.01)	(0.01)
Own car	-0.03	-0.11	0.02	-0.01
	(0.03)	(0.07)	(0.07)	(0.03)
Own house	0.00	-0.01	-0.01	-0.01

	(0.01)	(0.02)	(0.02)	(0.01)
Constant	0.08*	0.35***	0.20**	0.12***
	(0.04)	(0.09)	(0.09)	(0.04)
Observations	8,514	8,505	8,474	8,499

Note: Clustered standard errors in parentheses. *p<0.1; **p<0.05; ***p<0.01

Table 24: ***LATE Wives' Decision Making***

	Dependent variable:			
	Children school	Children doctor	House repairs	Children spending
Treatment	0.00	(0.00)	0.01	0.02*
	(0.01)	(0.01)	(0.01)	(0.01)
Indigenous Heritage	-0.01	-0.02***	-0.02***	-0.03***
	(0.01)	(0.01)	(0.01)	(0.01)
Sex	0.08	0.05	0.03	0.03
	(0.06)	(0.04)	(0.04)	(0.04)
Years of education	0.00	0.002**	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Age	0.00**	0.00	0.00	0.00*
	0.0)	0.00	0.00	0.00
Labor Participation	-0.02	-0.03**	-0.04***	-0.06***
	(0.02)	(0.01)	(0.01)	(0.01)
Married	0.00	(0.00)	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.01)
Yearly Household Income	0.01***	0.00	0.00	0.01**
	(0.00)	(0.00)	(0.00)	(0.00)
Own land	-0.01	-0.01	-0.02**	-0.01*
	(0.01)	(0.01)	(0.01)	(0.01)
Own animals	-0.02***	-0.01*	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)
Have electricity	-0.01	-0.00	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)
Own TV	-0.01	-0.01	-0.01	0.00

	(0.01)	(0.01)	(0.01)	(0.01)
Own radio	0.01	0.00	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.01)
Own car	0.02	0.03**	-0.01	-0.02
	(0.03)	(0.02)	(0.03)	(0.02)
Own house	0.02	0.01	0.01	0.00
	(0.01)	(0.01)	(0.01)	(0.01)
Constant	-0.04	0.45***	0.37***	0.41***
	(0.05)	(0.03)	(0.05)	(0.04)
Observations	8435	8458	8492	8461

Note: Clustered standard errors in parentheses. *p<0.1; **p<0.05; ***p<0.01

Table 25: ***LATE Husbands' Decision Making***

	Dependent variable:			
	Children school	Children doctor	House repairs	Children spending
Treatment	0.00	0.01	-0.01	-0.03
	(0.01)	(0.01)	(0.02)	(0.02)
Indigenous Heritage	0.04***	0.03***	0.04**	0.05***
	(0.01)	(0.01)	(0.02)	(0.01)
Sex	0.02	0.00	0.04	0.02
	(0.06)	(0.05)	(0.07)	(0.06)
Years of education	0.00*	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Age	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Labor Participation	0.02	0.02	0.04**	0.06***
	(0.02)	(0.01)	(0.02)	(0.02)
Married	0.00	0.00	-0.01	-0.01
	(0.01)	(0.01)	(0.02)	(0.01)
Yearly household income	0.00	0.00	0.00	-0.01*
	(0.00)	(0.00)	(0.01)	(0.01)
Own land	0.01	0.00	0.02	0.00

	(0.01)	(0.01)	(0.01)	(0.01)
Own animals	0.00	0.00	-0.02	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)
Have electricity	-0.02*	-0.02	-0.02	-0.02
	(0.01)	(0.01)	(0.02)	(0.01)
Own TV	0.01	0.01	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)
Own radio	0.00	0.00	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.01)
Own car	0.03	0.00	0.05	0.06
	(0.04)	(0.05)	(0.07)	(0.05)
Own house	-0.02	0.00	-0.02	0.00
	(0.01)	(0.01)	(0.02)	(0.02)
Constant	0.07	0.07	0.25**	0.19**
	(0.05)	(0.06)	(0.10)	(0.08)
Observations	8435	8458	8492	8461

Note: Clustered standard errors in parentheses. *p<0.1; **p<0.05; ***p<0.01

Table 26: ***LATE Effects Female Autonomy***

	Dependent variable:			
	Women control cash	Go out alone	Partner's Permission	Husband women's cash
Treatment	0.06**	0.03*	0.00	-0.01*
	(0.03)	(0.02)	(0.01)	(0.01)
Indigenous heritage	-0.07***	0.00	0.00	0.00
	(0.02)	(0.01)	(0.01)	(0.00)
Sex	0.07	0.00	0.07	-0.03***
	(0.07)	(0.05)	(0.05)	(0.01)
Years of education	0.00	0.00	0.00	0.00**
	(0.00)	(0.00)	(0.00)	(0.00)
Age	0.00	0.00***	0.00	-0.00**
	(0.00)	(0.00)	(0.00)	(0.00)

Labor Participation	0.00	-0.01	-0.01	0.00
	(0.03)	(0.02)	(0.01)	(0.01)
Married	0.02	0.02***	0.00	0.01***
	(0.01)	(0.01)	(0.01)	(0.00)
Yearly household income	0.00	-0.01***	0.00	0.00
	(0.01)	(0.00)	(0.00)	(0.00)
Own land	-0.01	-0.01	0.00	-0.01
	(0.01)	(0.01)	(0.01)	(0.00)
Own animals	-0.01	-0.02*	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.00)
Have electricity	-0.03	-0.03**	-0.02**	0.00
	(0.02)	(0.01)	(0.01)	(0.01)
Own TV	0.01	0.00	-0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)
Own radio	-0.01	0.01	0.00	0.01
	(0.01)	(0.01)	(0.01)	(0.00)
Own car	0.06	0.00	0.02	0.01***
	(0.07)	(0.06)	(0.02)	(0.00)
Own house	0.01	-0.01	0.00	0.01
	(0.03)	(0.01)	(0.01)	(0.01)
Constant	0.32***	0.11	0.01	0.02
	(0.10)	(0.08)	(0.04)	(0.02)
Observations	8435	8458	8492	8461

Note: Clustered standard errors in parentheses. *p<0.1; **p<0.05; ***p<0.01

Chapter 3

Gender Gaps in Education and Changes in Union Formation: The Case of Bolivia, Colombia, and Peru

Introduction

Women's educational status has radically changed in recent decades. Across the world, women have not only closed the gender gap in education but have surpassed men in all levels of schooling (Diprete and Buchmann 2006; Goldin et al. 2006; Grant and Behrman 2010). This trend represents the reversal of a once persistent pattern of gender stratification, impacting women's labor market position, health outcomes, and family formation behavior (Buchmann and DiPrete 2006; Goldin et al. 2006; Schwartz and Han 2014). Because education plays a pivotal role in individuals' marriage timing and partner selection (Blossfeld 2009), scholars have been especially interested in the implications of this population shift for union formation (Van Bavel et al. 2018).

The consequences of women's increased schooling on marriage seem to vary widely depending on a country's level of gender inequality (Blossfeld and Huinink 1991; Raymo and Iwasawa 2005), the type of cultural norms guiding marital selection (Lin et al. 2020), and the extent of female advantages in education (De Hauw et al. 2017). For instance, while greater schooling is associated with higher chances of marriage for women in the U.S and several European countries (De Hauw et al. 2017; Perelli-Harris and Lyons-Amos 2016; Sweeney 2002), it drastically reduces

the odds of entering a union in East-Asian societies (Raymo and Park 2020; Raymo 2003). In terms of partner selection, researchers have found that in a wide range of contexts, including India, Sweden, and the U.S, the rise in women's educational attainment is linked with the "end of hypergamy"—those marriages in which husbands are more educated than wives (Esteve et al. 2016; Erát 2021)—and an increase in the proportion of women "marrying down" in education (Lin et al. 2020). On the other hand, recent scholarship in West Africa shows that improvements in women's educational opportunities are associated with a higher prevalence of hypergamous unions, augmenting status differences between spouses (Lopus and Frye 2020).

This chapter extends this literature by studying the case of Bolivia, Colombia, and Peru, which have experienced some of the greatest growth rates in women's education in the last few decades (Grant and Behrman 2010). Unlike other regions in the Global South, several Latin American countries have reached a point where there is a female advantage in all levels of instruction, and a handful of others are on the verge of closing all gender differences in educational attainment (Esteve et al. 2016). Despite these advances in educational status, the link between women's education and economic returns—a critical feature for reducing intra-marital inequalities in other contexts (Sweeney and Cancian 2004; Oppenheimer 1988)—is persistently weak in the majority of these countries (Gasparini and Marchionni 2017). Moreover, patriarchal norms—including gendered views of the division of labor in families, the expectation of submissive behavior from women, and hypergamy ideals in marital selection—still are pervasive in the region (Ingoldsby 1991; Orgill and Heaton 2005).

Under these contextual conditions, does the increase in women's education relative to men reduce enduring gender asymmetries in union formation? To answer this question, I assess two aspects of the marriage[1] process. First, I ask whether the relationship between women's education and *marriage entry* changes as cohorts of women gain in educational attainment. In particular, are highly educated women less likely to marry as gender gaps are overturned? Second, I exam-

[1]When using the term *marriage* in this chapter, I will also be referring to non-marital unions. In Latin America, both types of unions have historically coexisted and are considered to have a similar social standing (Castro Martín 2002; Esteve et al. 2012c).

ine whether *marriage patterns* and *propensities*—social norms, preferences, and gendered ideals guiding marital matches (Schoen and Kluegel 1988)—have shifted towards greater gender symmetry as cohorts of women become more educated. In this analysis, I focus on Bolivia, Colombia, and Peru, which present varying degrees of gender gaps in education. While Colombia displays female educational advantages at all levels of schooling, in Bolivia and Peru, younger cohorts of women just reached educational parity (IPUMS 2020). I choose these cases, precisely because of their variation in educational context, amidst the prevalence of common cultural norms regarding gender and family. To empirically assess changes in union formation, I combine data from the Demographic Health Surveys with national censuses (IPUMS 2020), spanning cohorts of women born between 1920 to 1985 in each country.

This chapter extends prior research in two key ways. First, most scholarship examining union formation and the reversal of gender gaps in education focuses on marriage entry *or* marital sorting. In this study, I analyze both processes in tandem as examining who exits or is left out of the marriage market is critical to understand the evolution of marital pairings under changing educational compositions. Second, this chapter includes the Latin American case in the scholarly debate about the reversal of gender gaps and its implications for marriage and family life. Despite that educational expansion in these countries dramatically improved women's educational attainment (Grant and Behrman 2010; Urbina 2018), we know very little about how these shifts are changing gender inequalities in union formation (see Esteve and López-Ruiz (2010) for an exception).

Theory and Prior Research

Since this study focuses on two dimensions of the union formation process, I first discuss prior scholarship on women's education and *marriage entry*. Then, I turn to the literature about changes in gender gaps in education and *marriage patterns*.

I. Women's Education and Marriage Entry

As female schooling grew across the world, different theoretical approaches predicted a decline in marriage entry among highly educated women. The "marital squeeze" or "marriage market mismatch" hypothesis views marriage entry as a function of the availability of *suitable* partners (Lichter et al. 1991; Schoen and Kluegel 1988). In many contexts, hypergamy—the expectation that men should marry down or at least equally in education and other markers of status—has been the guiding norm defining the suitability of marriage matches (Basu 1992; Bourdieu 2007; Esteve et al. 2016). This approach posits that in contexts where these norms are prevalent, marriage becomes less feasible for highly educated women as gender gaps are overturned (Blossfeld 2009; Raymo and Iwasawa 2005). Thus, if there are not enough men equally or more educated in the marriage market, then a higher proportion of educated women will remain single.

On the other hand, the "economic independence" hypothesis also predicts a decline in marriage rates among highly educated women (Oppenheimer 1988), albeit due to a vastly different mechanism. From a gender-specialization perspective (Becker 1993), as higher education increases women's economic resources, marriage becomes a less attractive arrangement——especially in societies where women carry the burden of housework and childcare (Cherlin 1992). As the gains from union formation decline, highly educated women will tend to "buy out" of marriage.

The extent to which these predictions hold varies by context. The empirical evidence in the United States and most European countries rejects both hypotheses, as being highly educated currently augments women's chances of entering marriage (De Hauw et al. 2017; Kalmijn 2013; Perelli-Harris and Lyons-Amos 2016; Sweeney 2002). Sociologists attributed this shift to a change in the "economic foundations of marriage" rooted in the increasing difficulties of sustaining a household based on gender specialization (Oppenheimer 1988, 1997). As a result, women's economic potential converged to be as important as men's earnings for union formation (Sweeney and Cancian 2004), leading to a positive relationship between schooling and marriage (Goldstein and Kenney 2001). In contexts of higher gender inequality, however, the evidence is consistent with the

predictions of both the "marital squeeze" and "economic independence" hypotheses. For example, studies in East Asian countries show that as women's education grew over time, schooling became negatively associated with union entry, while men's education kept being an attractive trait in the marriage market (Raymo and Iwasawa 2005; Raymo and Park 2020; Yu and Xie 2015).

Following this evidence, some contextual conditions in Bolivia, Colombia, and Peru lead to hypothesize that increases in women's schooling should lead to later and less marriage among the highly educated. The prevalence of hypergamy norms (García and de Oliveira 2011) and the rejection towards couples that deviate from this expectation (Chant 2002; Menjivar 2011) makes the scenario of a "marital squeeze" feasible, especially in Colombia where there is a female educational advantage in all levels of instruction. Likewise, the extent of gender inequalities in the home and the"double-shift" that most working women endure may lead some to "buy out" of marriage (Safa 1995; García and de Oliveira 1994, 2011). On the other hand, the weaker link between women's schooling and economic returns in these countries limits the applicability of the economic independence hypothesis (Gasparini and Marchionni 2017; Villanueva and Lin 2020). This fact constrains the extent to which highly skilled women can forgone marriage, and in turn, make it an attractive arrangement from a gender specialization perspective (Parrado and Zenteno 2002).

Surprisingly few studies have examined over time changes in the relationship between women's schooling and marriage timing in Latin America. The most comprehensive study to date by Ganguli et al. (2014), investigates union formation across several countries—including Bolivia, Colombia, and Peru—and finds that between 1990 and 2000, highly educated women were increasingly less likely to enter a union (Ganguli et al. 2014). Likewise, in Mexico Kroeger et al. (2015) show that later cohorts of college-educated women were more prone to postpone marriage, a pattern they did not observe for earlier cohorts of equally educated respondents. Yet, other scholars have found stability in union timing, even among highly educated women in the region (Esteve et al. 2013; Parrado and Zenteno 2002). The variation in data sources, sample selection, and cohorts included in these studies certainly explain some of these mixed findings. Nevertheless, they also offer a compelling reason to investigate further the relationship between female schooling and marriage

entry in the Latin American context.

II. Women's Education and Marital Patterns

Shifts in the gender gap in education are also likely to change marital patterns or "who marries whom." This chapter focuses on two types of mechanisms through which changes in marital patterns may occur: (1) changes in the educational composition and (2) changes in marriage propensities.

Changes in Educational Composition

Generally, educational expansion processes impact marriage markets by shifting the availability or scarcity of individuals with specific educational credentials (Schoen and Kluegel 1988). For instance, in countries where educational expansion favored men over women, there tended to be a higher proportion of hypergamous unions because of the gendered distribution of educational attainment (Lopus and Frye 2020). As gender gaps in education started to diminish, scholars hypothesized that increases in women's schooling relative to men would inevitably lead to the "end of hypergamy" in union formation (Van Bavel et al. 2018). The specific marital pairings predicted to grow due to this decline depend on the extent of female educational advantages (Schwartz and Han 2014). In contexts where men's and women's education is equally distributed, we should expect to see an increase in educational homogamy. Meanwhile, in places where women surpassed men's educational attainment, there should be a rise in hypogamy (unions where wives have more schooling than their husbands). Importantly, as these predictions are a function of changes in educational composition only, they may arise without any relevant transformations in norms guiding partner choice (Grow and Bavel 2015).

According to this mechanism, we should see a decline in hypergamy among younger couples in Bolivia, Colombia, and Peru, given the over time increase in the availability of educated women in these marriage markets. However, because there is significant temporal variation in the level of female/male advantages across these contexts, the specific marital pairings that may increase

as hypergamy declines should also vary. In Colombia, where women have had an advantage in education since earlier cohorts (more in Background section ahead), we should observe an over time increase in hypogamous unions. In Bolivia and Peru, where men have had an educational advantage until recently, we are more likely to observe a rise in educational homogamy as these gender gaps shrink over time. The existent evidence on marital pairings is generally consistent with these predictions. A seminal study by Esteve et al. (2016) found a decrease in educational hypergamy in most countries where women have made considerable gains in schooling, including Bolivia, Colombia, and Peru.

Nevertheless, it is not clear how marital pairings would shift in the scenario where increases in women's education also depress marriage rates among highly educated women. In the United States, Mare (1991) hypothesized that such a scenario would increase homogamy among more-educated couples. Alternatively, it could also lead to increases in hypergamy in contexts where women's retreat from the marriage market is considerable. This issue illustrates the need to evaluate both marriage entry and marital pairings to achieve a thorough understanding of union formation processes and women's schooling.

Changes in Propensities

Increases in women's education may also transform marital pairings by shifting marriage "propensities". Propensities can be understood as social norms and gendered ideals guiding the desirability or rejection of specific marital matches (Schoen and Kluegel 1988). These expectations operate through individuals' romantic preferences, familial interventions, and general beliefs about "who should marry whom" (Kalmijn 1998). As mentioned, the hypergamy norm has been a central organizing principle of marriage markets, historically perpetuating gender asymmetries in the process of union formation (Esteve et al. 2016; Mohanadoss 1995). In societies where this norm is prevalent, men would generally avoid to "marry up" in education or other markers of status, even if that implies remaining single (Bourdieu 2007).

With changes in women's education, different arguments predicted that the hypergamy norms

would increasingly erode, leading to a rise in homogamy and/or hypogamy propensities. For instance, modernization theories claimed that women's greater access to resources would introduce gender-egalitarian orientations in the process of spouse selection (Caldwell 1976; Jackson 1998), leading to greater gender symmetry in marital pairings. Others posited that women's gained economic autonomy would enable them to consider other traits than just men's education, choosing to marry individuals based on other characteristics that are not necessarily correlated with higher status (Fernández et al. 2005). Like marriage-entry theories, these hypotheses rest on the assumption that gains in women's education lead to greater economic power. Yet, in contexts where this link is weaker, it remains an empirical question whether we observe a shift towards more gender symmetry in marriage propensities.

Background: Gender Gaps and Family Change in Bolivia, Colombia, and Peru

In the last decade, women in Latin America have either equalized or surpassed men's educational attainment (Esteve et al. 2012b). Currently, the gender gap in education has been reversed in Brazil, Colombia, Ecuador, the Dominican Republic, and Venezuela (Esteve et al. 2012b). These trends are driven by decades of growth in educational expansion, especially by implementing compulsory school reforms that have significantly increased girls' educational attainment (Grant and Behrman 2010; Urbina 2018). Bolivia, Colombia, and Peru are no exceptions. Figure 1 shows the evolution of the gender gap in these three countries (Panel A). We can see that women had made significant gains in years of education, to the extent that younger cohorts in Bolivia and Peru are close to equalizing men's schooling, while Colombian women have had an advantage in education since cohorts born in the 1950s.

[Figure 1 here]

Panel B illustrates a similar story by plotting over time trends in the Female Advantage Index, developed by Esteve et al. (2012b). This index measures the level of aggregated gender asymmetry in educational attainment at the country level. It varies from full male advantage (0) to full female advantage (1), where 0.5 indicates no gender differences. As shown, this figure displays significant cohort and cross-country variation in the level of male/female advantage. For instance, while 1950 cohorts in Bolivia and Peru exhibited acute male advantages in schooling, Colombian cohorts had already attained gender parity. Lastly, Panels C and D show that the overturn of gender gaps in education are especially striking in tertiary education[2], where women now represent over 50% of the total student population in all countries.

As these educational trends progressed, women in Bolivia, Colombia, and Peru also increased their participation in paid employment. Between 1990 and 2015 female labor participation raised from 41% to 63% in Bolivia, from 39% to 58% in Colombia, and from 38% to 67.5% in Peru (World Bank Group 2019). Despite these gains, women's participation in paid employment remains well below the average of industrialized countries (i.e., 75% in the U.S). Significant gender pay gaps (Villanueva and Lin 2020), the double-burden of domestic responsibilities (Chant 2002), and the dominance of male-breadwinner norms (García and de Oliveira 2011) are some of the factors explaining these figures. The lower levels of female employment also illustrate the weaker relationship between women's education and their economic status across the region (Torche 2010). As mentioned, this contextual feature may limit the extent to which gains in schooling increase women's economic independence and their ability to "buy out" from marriage (Oppenheimer 1997).

Lastly, while women have experienced major transformations in their education and labor force participation, some aspects of their demographic behavior have remained surprisingly unaltered in the last few decades. For instance, rapid fertility declines in Latin America co-occurred with stable trends in the timing of childbearing and union formation (Esteve and Florez-Paredes 2018; Rosero-Bixby et al. 2009). Table 2 shows that in Bolivia, Colombia, and Peru, the mean

[2]Figure 2 shows in detail how gender gaps in education vary across each level of attainment.

age at marriage has remained relatively unchanged across cohorts. However, recent scholarship has also found signs of change among younger women. Despite that marriage remains a nearly-universal institution (Fussell and Palloni 2004), the proportion of women never married at age 35 has continuously increased across cohorts (See Table 2). Likewise, younger Latin American women are also waiting longer to become mothers, a shift that has been especially driven by the highly educated (Rosero-Bixby et al. 2009).

Research Design

Data and Sample

For this study, I used two sources of data. For most analyses concerning marriage timing, I rely on Demographic Health Surveys (DHS) (Heger Boyle et al. 2019). The DHS is a nationally representative household-based survey centered on reproductive and sexual health, children's health, and fertility. Crucially, these surveys contain retrospective measures on age at first marriage or cohabitation for all female respondents who have ever been in a union. I pooled all surveys collected between 2000 and 2016 and included all women who were between 20 to 49 years old at the time of each survey round [3]. To examine how marriage timing changes as women increase their schooling, I generated 10-year birth cohorts capturing different stages of the gender gap in education in each country. The characteristics of the analytical sample are described in Table 1. Across countries, the average respondent had between eight to nine years of schooling. At the time of the survey, around 70% of respondents were married or cohabiting in Bolivia and Peru, while 63% were doing so in Colombia. Amongst those respondents that had ever been married, the average age at marriage was 20 years old in all countries.

[Table 1 here]

[3]For Bolivia, I used the 2003 and 2008 DHS, for Colombia, I used the 2000, 2005, 2010, and 2015 DHS, and for Peru, I used the 2004, 2007, 2009, 2010, 2011, 2012 DHS.

To analyze marital pairings across cohorts, I use the Integrated Public Use Microdata Series (IPUMS) census samples (IPUMS 2020). For each country, I use census rounds from 1970 until the most recent census collection.[4] Following previous studies, I restrict this analysis to the sub-sample of prevailing marriages and cohabitating couples (Schwartz and Mare 2005; Torche 2010). Ideally, I would focus on newlyweds as prevailing unions are subject to different sources of bias, but unfortunately, that type of data is not available to conduct a comparative project such as this one. All censuses include the age and educational attainment of each spouse. To analyze over time trends in marital pairings, I generated 10-year birth cohorts according to wives' year of birth, spanning from 1920 to 1980.

In this study, I do not distinguish between cohabitation and marriage. In Latin America, both types of unions have historically coexisted and are considered to have a similar social standing (Castro Martín 2002; Esteve et al. 2012c). In most countries[5], non-married couples usually have children together, and cohabiting remains a common arrangement at later stages of the life cycle (Castro Martín 2002; Palloni et al. 1996). Moreover, cohabitation represents almost two-fifths of all unions in Bolivia, Colombia, and Peru, and importantly, it is not stratified by education (Esteve et al. 2012c).

Measures

For all analyses, the most important measure is women's educational attainment and, if pertinent, their spouses'. Fortunately, both the DHS and IPUMS-International contain harmonized educational attainment variables for all samples across countries. I measure educational attainment using four categories: 1 "Less than Primary (P-)", 2 "Completed Primary (P)", 3 "High School Completed (HS)", and 4 "College Degree or Higher (C)". To analyze marital pairings, couples were classified as "homogamous" when both spouses belong to the same educational category,

[4]For Bolivia, I used the 1976, 1992, 2001, and 2012 national census, for Colombia, I used the 1973, 1985,1993, and 2005 census, and for Peru, I used the 1993 and 2007 census.

[5]Excluding the more economically advanced countries—Argentina, Chile, and Uruguay.

"hypergamous" when husbands had a higher level of schooling than his wife, and "hypogamous" when the wife had a higher level of education than her husband.

Following a classic demographic approach to the study of social change (Ryder 1965), I use women's birth cohorts as a proxy for the different stages of a country's gender gap in education. Although Colombian women equalized men's schooling in earlier birth cohorts than Bolivia and Peru, the three countries share comparable education levels among the general population and have a similar pace of educational expansion. As noted earlier, they all implemented compulsory schooling reforms between 1991 and 1994, which significantly increased women's educational attainment in all countries.

Lastly, hazard models control for a series of measures known to affect both women's educational attainment and union formation. In particular, all models incorporate women's current region of residence and a dichotomous indicator measuring whether they live in a rural area. This is an important control due to the wide urban/rural disparities throughout educational expansion in Bolivia, Colombia, and Peru (UNESCO 2004). Additionally, because indigenous communities in Bolivia and Peru tend to live in rural areas (IPUMS 2020), I also include respondent's ethnicity in most models. Ethnicity is measured as a categorical variable which included "Spanish/White," "Quechua," "Aymara," and "Other Indigenous" for the case of Bolivia and Peru, and "Spanish/White," "Indigenous," "Afro-descendant" and "Other" for Colombia.

Analytical Strategy

This research follows two analytical strategies. First, to examine the relationship between women's educational attainment and their transition to marriage, I estimate a series of nested discrete-time hazard models. Second, to analyze marital pairings over time I turn to log-linear models.

I. Discrete-time Hazard Models

Generally, hazard models estimate whether and when an event, such as marriage, occurs.

This strategy allows me to assess the effects of fixed and time-varying covariates on marriage entry, including duration of exposure, without the assumption of proportionality (Allison 1995). Because my goal is to analyze *changes* in the relationship between education and marriage over time, modeling strategies assuming proportional hazards were not suitable.

To simplify the estimation of these models, I transformed my data into person-year records. Each observation represents one person-year between the initial exposure to the risk of marriage and the event's occurrence (marriage), or if never married, the end of the survey. I defined initial exposure as age 15 as this age signifies the transition to womanhood in Latin American culture. Moreover, I decided to split the DHS respondents into two groups. In Bolivia, I compared women born between 1950 and 1960 with those born between 1970 and 1980[6]. In Colombia and Peru, I compared respondents born between 1950 and 1960 to those born between 1975 and 1985. This strategy has been used in prior analyses of women's schooling and marriage timing, (Raymo 2003; Yu and Xie 2015) to ensure a parsimonious and intuitive way of assessing the assumption of time-invariant effects of women's schooling.

The dependent variable in all models is an indicator equal to '1' if the marriage took place in the interval between two given years and '0' if it did not. Models are estimated for each country separately. The specification of the baseline model is the following:

$$\ln[p_{it}/(1-p_{it})] = \beta_1 EXP_i(t) + \beta_2 EDU_i + \beta_3 COHORT_i + \beta_4 \mathbf{X_i} + \epsilon_{ic} \quad (1)$$

Where $\ln[p_{it}/(1-p_{it})]$ is the log-odds of marriage of individual i at exposure duration t, conditional on not getting married during $t-1$. $EXP_i(t)$ represents the duration of exposure to marriage, which starts at age 15. Following previous studies, I model exposure using a linear spline with knots at ages 19, 25, and 30 (Raymo 2003; Yu and Xie 2015). EDU_i represents the educational attainment of woman i, $COHORT_i$ denotes the cohort membership of woman i, $\mathbf{X_i}$ is a vector of covariates for individual i, and ϵ_{ic} are standard errors clustered by survey cluster.

[6]Because of sample size restrictions, I could not use a later cohort as a comparison.

From this baseline specification, I estimate a series of nested models that sequentially build-up in complexity. I start by including an interaction term between exposure and education (Model 2) to test how the marriage curve varies by women's schooling. In Model 3, I add an interaction between women's education and birth cohort to examine if this relationship has changed over time. To specifically test if the marriage curve changes by cohort, Model 4 includes an interaction between exposure and cohort. Lastly, Model 5 includes a full interaction term between exposure, education, and cohort to examine if women's increased schooling is associated with less or later marriage.

II. Log-linear Models

My second analytical strategy examines marital pairings and the propensities underlying these trends. To this end, I use log-linear models for contingency tables. The goal of these models is to accurately describe the patterns of association between categorical variables without over-fitting the data (Agresti 2002). In this context, they provide estimates of the association of couples' education while controlling for changes in the marginal distribution of educational attainment. Substantively, log-linear models disentangle whether observed changes in marital pairings are solely due to transformations in the composition of marriage markets, or they also reflect underlying changes in the social propensities guiding who marries whom (Schwartz and Mare 2005).

To estimate these models, I proceeded as follows. First, for each country, I analyze three-way tables by cross-classifying husband's education (5 categories), wife's education (5 categories), and wives' birth cohort (6 categories). In particular, I rely on different specifications of the *homogamy* model, which examines if individuals are more likely to marry partners with their same level of schooling (Powers and Xie 2000). The canonical version of this model assumes mating patterns are captured by the marginal distribution of husbands' and wives' schooling, plus one three-way interaction that models educational homogamy across the main diagonal of the contingency table for each country (Powers and Xie 2000). Formally,

$$\ln(F_{ijk}) = \mu + \mu_i^H + \mu_j^W + \mu_k^C + \mu_{ij}^{HW} + \mu_{ik}^{HC} + \mu_{jk}^{WC} + \gamma_{ol}^{OC} \quad (2)$$

Where *H* is husband's education, *W* is wife's education and *C* is birth cohort. In this model a single parameter γ_{ol}^{OC} represents the changes in the log-odds of *educational homogamy* in cohort *l* relative to the baseline cohort. *O* is equal to '1' if the husband's education is equal to his wife's education, and '0' otherwise. Other versions of this model allow for more complex patterns of educational pairings. For instance, the unconstrained homogamy model allows the strength of homogamy to vary across levels of educational attainment, while the asymmetric model incorporates parameters to capture hypergamy and hypogamy patterns along the minor diagonals of the contingency table (Powers and Xie 2000; Schwartz and Mare 2005).

In these analyses I test several of these model specifications—each corresponding to a different hypothesis regarding the mating structure across birth cohorts—and adjudicate between them on the basis of both substantive reasons and goodness of fit statistics[7]. Alternatively, I also applied a Lasso regularization approach (Bucca and Urbina 2019) to identify patterns of association not covered by traditional log-linear specifications. Once the best fitting model is selected for each country, I proceed to plot the main estimates of each log-linear model.

Main Findings

I. Women's Schooling and Changing Patterns of Marriage Entry

As cohorts of women gain in educational attainment, does the relationship between schooling and marriage entry change? To start answering this question, I plot Kaplan-Mier survival estimates for the transition to first marriage by cohort. Marriage curves in Figure 3 show that among earlier cohorts (1950-1960), women with primary and secondary schooling married sooner than their

[7] AIC, BIC, Dissimilarity Index and Likelihood Ratio.

more educated counterparts. This gap starts to decline from age 25 onward, as highly educated women start to "catch up" in terms of marriage entry. By age 35, the gap across groups is close to zero in all countries.

[Figure 3 here]

Transition curves for later cohorts (right panel) reveal larger differences across educational groups. This figure depicts that a higher percentage of women with tertiary education are postponing marriage, and by age 35, a non-negligible share of them remain unmarried. Simultaneously, a higher proportion of women with primary and secondary schooling are entering marriage at younger ages, resulting in greater differences in union timing across educational groups. Taken together, these plots suggest that in later cohorts, highly educated women are marrying later, and possibly, less than their counterparts with primary and secondary schooling.

In order to further examine these preliminary findings, I now turn to discrete-time hazard models. These models allow me to analyze whether and when women marry by considering their duration of exposure to marriage and another set of relevant covariates.

I start by presenting the results of the baseline model for women's marriage entry in each country. As shown in Table 3, women with tertiary education are significantly less likely to enter a union than those with less than primary schooling. Higher education reduces the odds of marriage by 69% (i.e. $1 - e^{-1.18} = .69$) in Bolivia and 68% in Colombia and Peru. Moreover, a high school degree only hurts women's likelihood of marriage in Bolivia (i.e., $1 - e^{-.34} = .29$), while completing primary schooling significantly increases the odds of marriage for Bolivian and Peruvian women. Table 3 also provides insights about marriage timing across these countries. Generally, exposure coefficients indicate that the likelihood of marriage is the greatest right after high school (ages 19 to 24), plateauing at ages 25-29, and then declining from age 30 onward. In Colombia, however, we see that this decline is interrupted by an increase in marriage risk at age 30 or more.

[Table 3 here]

From this baseline specification, I estimated a series of nested models that sequentially build-up in complexity. In order to simplify the exposition of results, I focus on two of these models[8]. Table 5 contains the results of Model 3, which examines how the relationship between women's education and marriage changes over cohorts and relaxes the assumption of proportionality. Results show that in Bolivia, Colombia, and Peru, later cohorts of highly educated women are less likely to marry than their equally educated counterparts from earlier cohorts. For instance, Bolivian graduates born in 1970-1980 were 86% $(1 - e^{-1.96})$ less likely to marry, while women born in 1950-1960 with the same schooling were a 71% $(1 - e^{-1.24})$ less likely to do so. Furthermore, the improvement of model fit[9] with the inclusion of these terms confirms that the relationship between women's education and marriage is not time-invariant.

To get a fuller picture of marriage entry, Model 5 fully interacts exposure to marriage, education, and birth cohorts[10] (See Table 7). Because this model contains several two-way and three-way interaction terms, I calculated the predicted probability of marriage at each level of educational attainment to help to interpret these coefficients[11]. In particular, reference women were white, from rural backgrounds, and born in the later cohorts (1970-1980 for Bolivia, 1975-1985 for Colombia and Peru).

[Table 8 here]

Table 8 contains these predicted probabilities. Across countries, we can see that union timing has not significantly changed for women with primary schooling, except for a reduction in marital risk after age 30. Meanwhile, the likelihood of marriage somewhat declines for women with secondary education, especially after age 25. However, it was among women with tertiary education where we see the most change. First, respondents in this group experienced a decline in marriage risk in almost all exposure categories. The biggest reductions occurred in the 25 to 29 age group

[8] The results of nested models 2 and 4 are found in Tables 4 and 6, respectively

[9] According to Chi-square tests comparing nested models.

[10] This model was preferred over simpler specification according to Chi-square tests in all countries.

[11] Other variables were held at specific values

and the more than 30 age group. Second, the extent of this decline varied across countries. Bolivian women experienced the strongest reductions in marriage risk, followed by Peruvian and then Colombian respondents. For instance, 1950 cohorts in Bolivia had a 0.53 chance of getting married after age 30, while 1970 cohorts only had a 0.22 chance. In Colombia, this change was somewhat less drastic, going from a 0.46 to 0.31 chance of marriage after 30. These predictions suggest that while in earlier cohorts, highly educated women "caught-up" on marriage at later ages, in later cohorts they are increasingly less likely to do so.

In sum, these analyses reveal that marriage entry has significantly shifted for highly educated women in Bolivia, Colombia, and Peru. As gender gaps in education are reduced and overturned, these women are less likely to get married—following a pattern that has been documented in other high-gender inequality contexts.

II. Women's Education and Marriage Patterns Across Cohorts

Changing Prevalence of Marital Pairings

Do we see changes in marital pairings as the education composition of marriage markets shift? Figure 4 plots the proportion of unions in each cohort according to spouses' education and whether they follow educational homogamy, hypergamy, or hypogamy pairings. Notably, these figures present a sharp decline in educational homogamy in all countries, from representing around 75% of all unions (85% in the case of Bolivia) to be slightly more than 50% of marriages. Moreover, the specific marital pairings that grew as homogamy declined varied across countries.

[Figure 4 here]

In Bolivia and Peru, the downturn in homogamy was coupled with a significant increase in hypergamy. Whereas in earlier Bolivian cohorts, only 12% of marriages followed this pairing, it

later represented approximately 35% of all unions. In Peru, we see a smaller rise in hypergamy, shifting from being 22% to 28% of all marriages. These findings contradict the prevailing argument that improvements in women's schooling necessarily lead to the "end of hypergamy." Indeed, as increases in hypergamy unfolded throughout Bolivia and Peru, women were significantly catching-up to men's schooling to the point of closing the gender gap among later cohorts.

On the other hand, marital trends in Colombia behave as expected. As female advantages grew, we see a modest rise in hypogamy and a decline in hypergamy. As Figure 4 shows, in later cohorts, hypogamy becomes the second most prevalent pairing after homogamy, representing 25% of all unions. The cross-over between hypergamy and hypogamy occurs in tandem with the reversal of the gender gap in schooling, clearly illustrating the role of female advantages in the population for patterns of who marries whom.

These descriptive figures suggest that given the declines in homogamy across countries, hypergamy seems to decrease only when women surpass men's educational attainment—such as in Colombia—and not necessarily when education is equally distributed across gender. However, the variation in marital patterns between these countries may also denote differences in propensities guiding marriage.

Changing Propensities in Marital Selection

To uncover whether overtime changes in women's education transformed *propensities* guiding union formation, I rely on log-linear models. This approach identifies underlying patterns of marital selection, net of the changes in educational distribution in each country.

Tables 9, 10 and 11 contain all the model specifications I examined per country and its corresponding goodness-of-fit statistics. In addition, model search with Lasso regularization did not yield substantively different specifications. The model that best-represented marriage patterns in Colombia and Peru includes an unconstrained diagonal parameter to model homogamy, varying for each educational level, and an additional parameter that models asymmetrical movements across

the minor diagonals. The asymmetrical nature of this parameter allows for hypergamy to be different from hypogamy in the model. In the case of Bolivia, the best fitting specification varies in one key aspect with Colombia and Peru, that is the additional parameter that models changes in hypergamy, and hypogamy is symmetrical[12]. The full results of these models are found in Tables 12 and 13.

Figures 5, 6 and 7 plot the coefficients of each preferred model by country. Estimates represent the log odds for each combination of partners' educational level relative to the baseline categories (i.e., both partners with "less than elementary" in 1920). Across contexts, we can see two major changes in marriage propensities across cohorts. First, there is a steep increase in marriages between college-educated individuals, as shown by the purple line in panel T in each figure. The rise in homogamy propensities among the highly educated is a trend found in many other countries, including China (Han 2010), Mexico (Esteve and McCaa 2007), and the U.S (Schwartz and Mare 2005). Second, there is a significant decline in homogamy propensities between partners with less than primary schooling (see red line, panel P-). However, note that homogamy remains the dominant convention among less-educated women overtime.

[Figures 5, 6 and 7 here]

That said, what has not changed in terms of marriage propensities is especially noteworthy. Overall, the tendency to marry individuals with the same schooling continues to be the main pairing convention at the extremes of the educational distribution in Bolivia, Colombia, and Peru. Notably, we do not see increases in hypogamous propensities among highly educated women in any country. This is particularly surprising in the case of Colombia, where women have had an advantage in education for several cohorts, and the proportion of hypogamous unions has increased. These results indicate that Colombia's rise in hypogamy is primarily a function of shifts in education distribution and not due to changes in propensities, empirically confirming Grow and Bavel (2015)'s

[12]The only goodness-of-fit statistics that disagreed with this conclusion was the BIC, but given the consistent agreement between the likelihood ratio (G2), Dissimilarity Index and the AIC, I decided to choose the constrained minor diagonal model.

findings using agent-based models.

Furthermore, in all countries, we observe that women with secondary schooling are most prone to marry men with tertiary education (purple line, panel S). This tendency to "marry up" has been the dominant propensity for women with secondary schooling across cohorts, demonstrating that hypergamy norms are not in decline. Lastly, among spouses with primary and less than primary schooling, we observe more fluid propensities. Whereas women with primary schooling in Bolivia and Colombia, and to a lesser extent in Peru, have continuously had higher odds of hypogamy (red line, panel P), women with less than primary also have higher odds of hypergamy. These findings suggest that among the least educated, marriage conventions are generally less rigid and that women do not experience a loss of status by "marrying up" or "down" within these categories.

Together, marital propensities have remained generally stable as women's schooling increased over cohorts. In particular, we do not observe an overtime decline in hypergamy propensities or notable increases in hypogamy—not even in Colombia, where women have surpassed men in all education levels. These results highlight the persistence of gender asymmetrical ideals in union formation in the Latin American context.

Discussion and Conclusions

The reduction and overturn of gender gaps in education have transformed union formation processes across the world (Esteve et al. 2016; Lin et al. 2020; Lopus and Frye 2020; Schwartz and Mare 2005). This study extends this literature by examining the case of Bolivia, Colombia, and Peru—three Latin American countries that experienced sizable growths in women's education. In particular, I analyze how increases in female schooling have changed both marriage entry and marital patterns, especially among highly educated women. Because whom individuals marry is partly a function of the availability of suitable patterns (Schoen and Kluegel 1988), these two dimensions of union formation must be examined in tandem to get a comprehensive assessment of how

changes in educational composition may impact marriage.

Consistent with prior evidence from high gender-inequality contexts, I found that more educated women were less likely to get married than their less-educated counterparts in all countries. Discrete-time hazard models show that this decline in marriage entry was the greatest for later cohorts of highly educated respondents. In particular, women with tertiary education experienced a reduction in marriage risk in almost all exposure categories, especially between the ages of 25 to 29 and after age 30. Whereas Bolivian women with tertiary education experienced the strongest reductions in their probability of marriage, this decline was smaller for Colombian respondents. These findings support the "marital squeeze" and the "economic independence" hypotheses, as both predicted marriage declines among highly educated women. It is outside the scope of this study to determine which of these two hypotheses better explains these findings, however, my results regarding marital pairings may shed some light on this question.

Marital patterns reveal that as women's education unfolded, educational homogamy—unions where spouses have the same schooling—strongly decreased in all countries. The type of marital pairings that gained in prevalence with this decline varied by educational context. In Colombia, where women surpassed men's educational attainment, hypergamy—marriages where men have more schooling than their wives—decreased over birth cohorts, while hypogamy grew and became the second most prevalent arrangement after homogamy. These trends follow the literature's expectation that increases in women's schooling would reduce the proportion of hypergamous unions, especially in contexts where gender gaps had already been reversed.

In contrast, in Bolivia and Peru—where later cohorts of women have closed the gender gap in schooling—we see an over time increase in hypergamy. Although homogamy continues to be the most prevalent arrangement, this growth in couples where men are "marrying down" deviates from the prediction that gains in women's schooling would inevitably lead to the "end of hypergamy" (Van Bavel et al. 2018). Notably, prior studies that have found decreases in hypergamy in most world regions (Esteve et al. 2016, 2012a), including Latin America, focus only on heterogamous

unions. This methodological decision seems to have critical substantive implications, as shown in this work and others studies investigating hypergamy as a proportion of *all* marital pairings (Lopus and Frye 2020).

This research also examined whether there were shifts in propensities guiding marital selection. Using log-linear models, I found that net of transformations in educational composition, there has been more stability than change in marital propensities. As observed in the United States (Schwartz and Mare 2005) and other Latin American countries (Esteve and McCaa 2007), the most notable change in marital sorting is the increase in educational homogamy at the top of the education distribution. As educational expansion unfolded, we see a strengthening of social barriers among the highly educated in all countries, converging to a pattern of assortative mating identified in other high education contexts in the region, such as Chile (Torche 2010).

That said, two key features of marriage selection have remained unchanged. First, hypergamy ideals do not seem to be in decline. In Bolivia, Colombia, and Peru, women with secondary schooling still are most likely to "marry up" in education, a tendency that has not decreased even has women equalized and outpaced men's schooling. Second, hypogamous propensities do not increase over birth cohorts, a finding that is particularly surprising in the case of Colombia. The latter suggests that most of the growth in hypogamous marriages in Colombia is explained by compositional changes rather than underlying shifts in propensities guiding marital selection. Together, this evidence reveals the endurance of gender asymmetrical norms, which show little sign of change even in contexts where women have had a female advantage in education over several birth cohorts.

In light of these findings, it seems likely that a lack of "suitable" partners explains some of the declines in marriage among highly educated women across countries. The persistence of hypergamy propensities, net of changes in educational distributions, indicate that "suitable" matches for women with a secondary degree or more, continue to be husbands that are equally or more educated. Therefore, as women catch up and surpass men's schooling, the numeric feasibility of

appropriate matches decreases, just like the "marital squeeze" hypothesis proposes. Future studies should empirically test this argument, following prior work conducted in Japan (Raymo and Iwasawa 2005) and South Korea (Raymo and Park 2020). In addition, the weaker connection between women's education and employment in these contexts—a feature of most Latin American countries—may be an important reason why we do not observe more profound changes towards gender symmetry in marital propensities. Scholarship in the U.S has identified women's gains in economic power as one of the main drivers of change in the foundations of marriage (Oppenheimer 1997; Sweeney 2002). Perhaps we will observe such transformations in the Latin American context only once schooling brings up significant economic opportunities for women.

In closing, some limitations of this study should be acknowledged. Due to data restrictions[13], my analysis of marriage entry included women only. A comprehensive portrait of changes in educational gender gaps and union formation should analyze how these transformations impact men's marriage risk and how the interaction between men's and women's mate preferences shape marriage market dynamics (Logan et al. 2008). Besides, this analysis focused on national contexts of union formation without further examining differences between urban and rural marriage markets. This is a relevant oversight as gender gaps favoring males were more persistent in rural settings, especially in Bolivia and Peru. Moreover, research has shown that other regional conditions such as local economic opportunities (Lichter et al. 1991) and urban/rural differences in gendered practices and beliefs (Malhotra 1997) play a fundamental role in the marital search of women. The implications of urban and rural differences for changes in union formation certainly merit further investigation. Lastly, this study's methodological approach cannot yield causal conclusions about the effect of women's education on union formation. The implementation of compulsory school reforms during the 1990s in most Latin American countries, including Bolivia, Colombia, and Peru, provides an ideal setup to test some of this study's findings in a causal framework.

[13]The DHS only included information about women's age at marriage/cohabitation.

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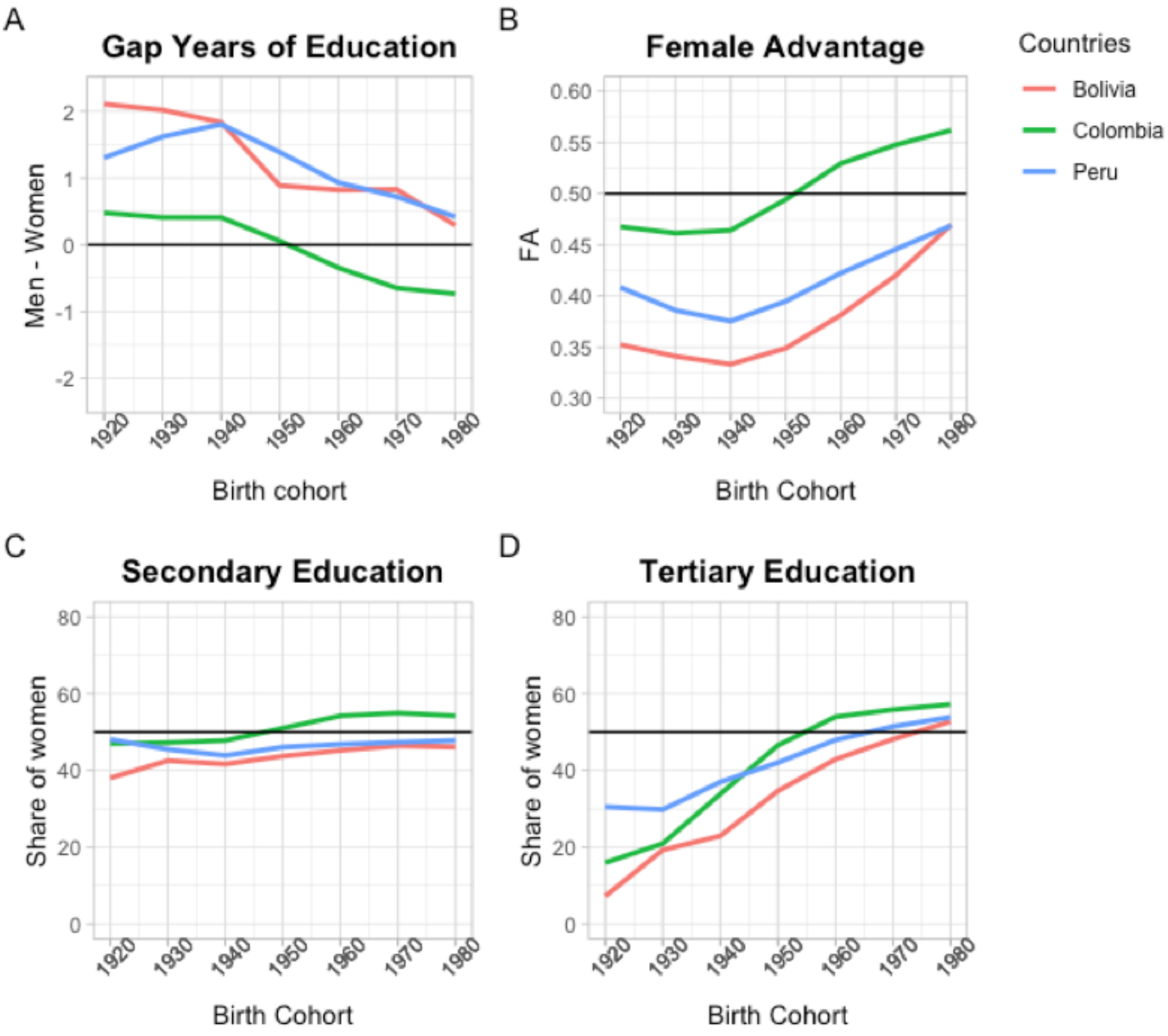
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Figures

Figure 1: Changes in Women's Educational Attainment



Note: Figures created by the author using Census data (IPUMS 2020).

Figure 2: Distribution of Educational Attainment by Gender and Cohort

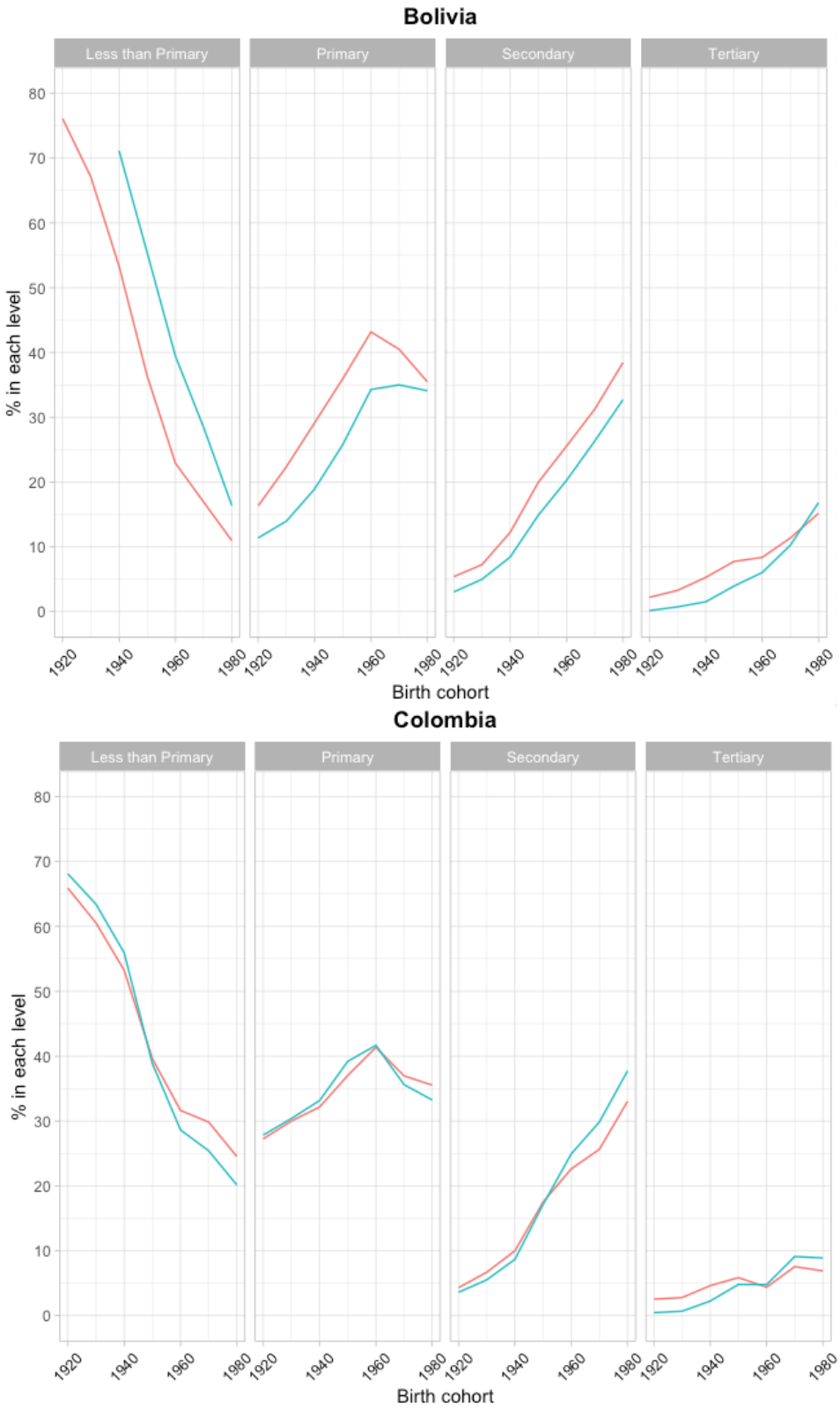
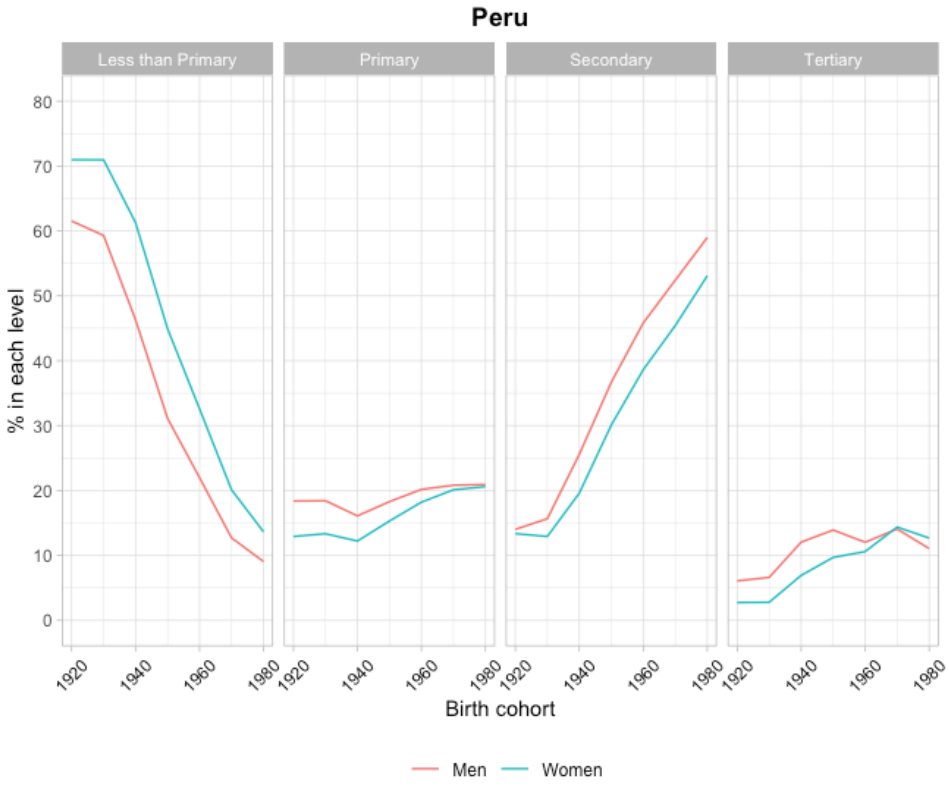
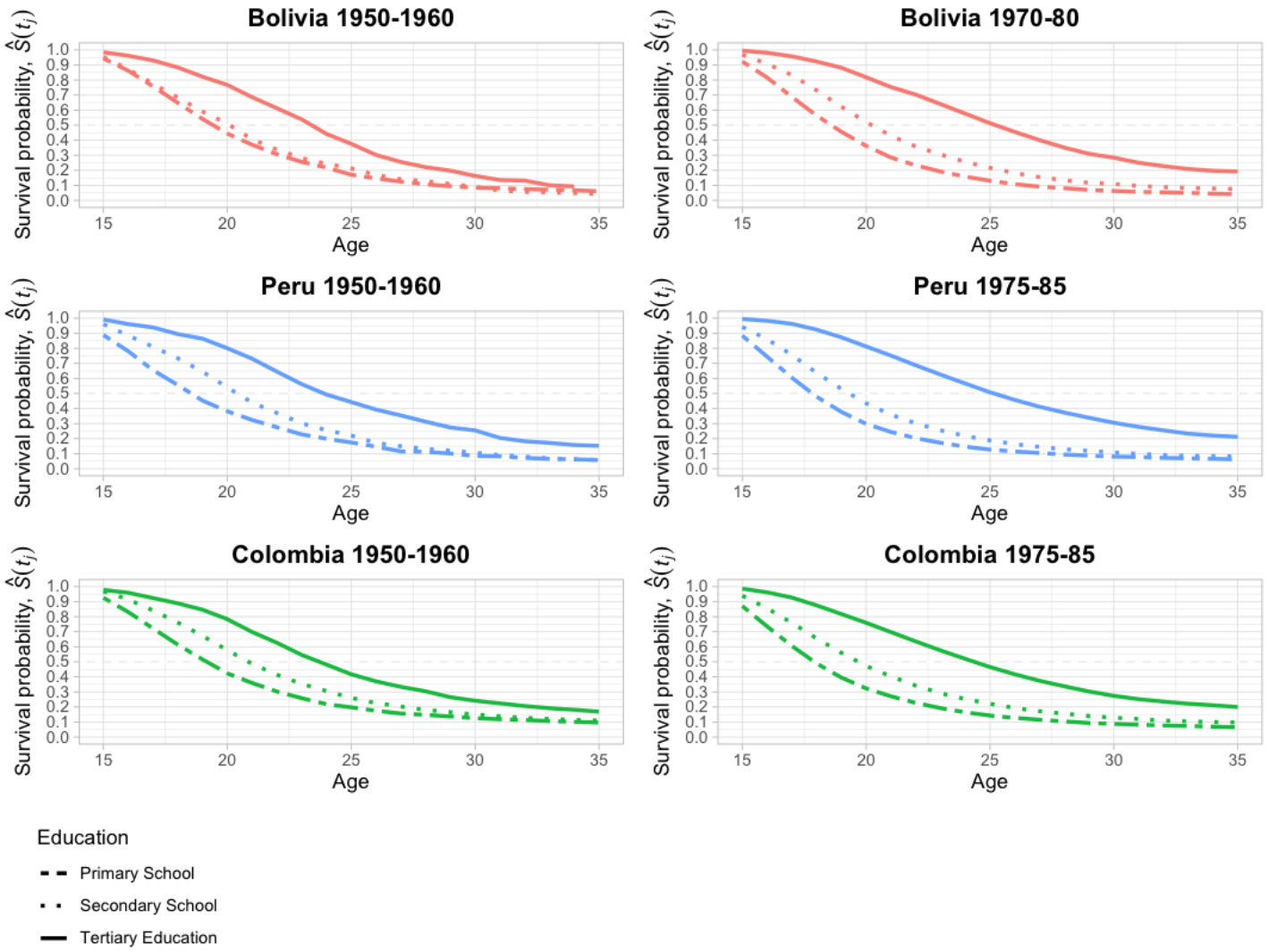


Figure 2: Distribution of Educational Attainment by Gender and Cohort



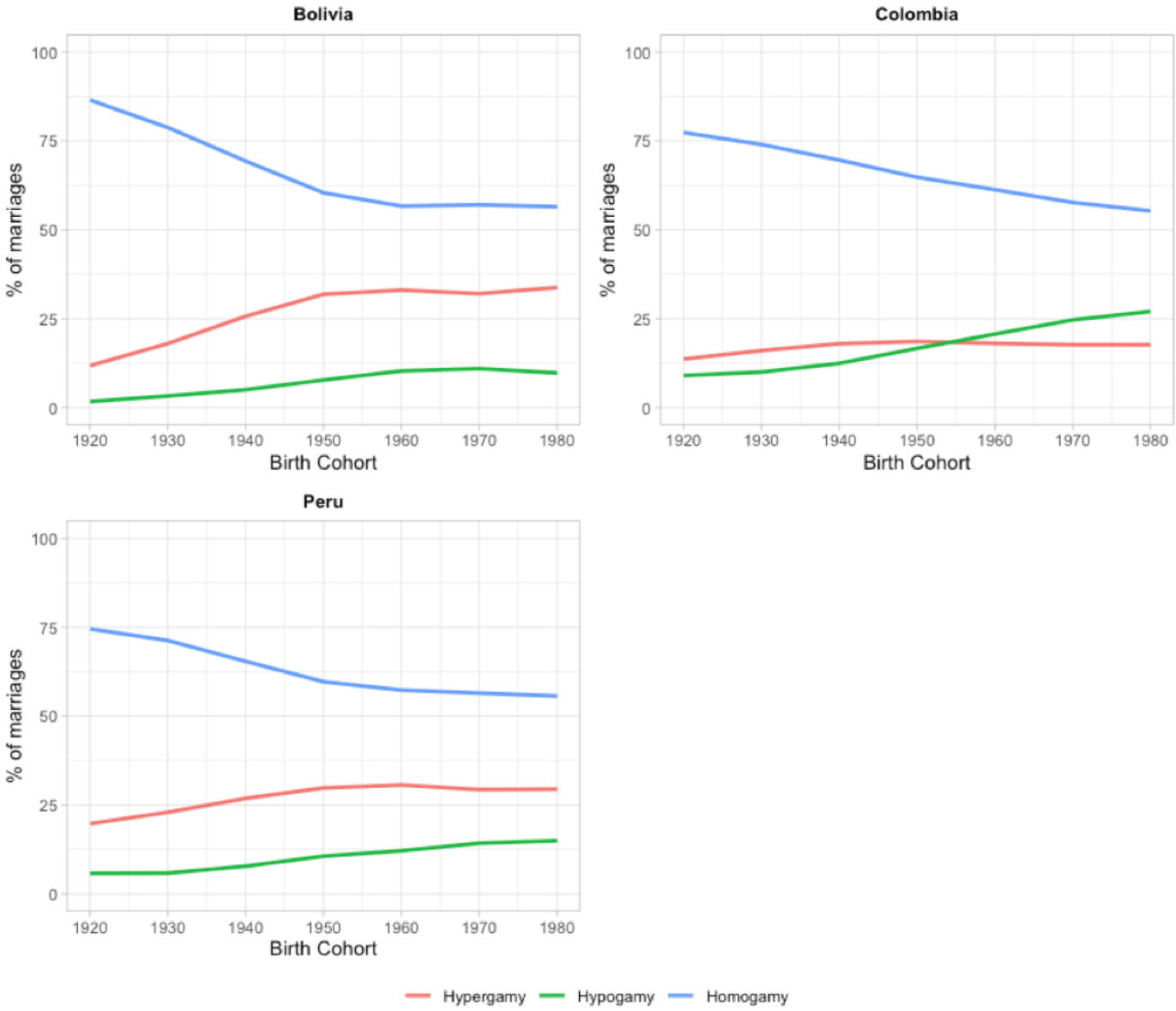
Note: Figures created by the author using Census data (IPUMS 2020).

Figure 3: Kaplan-Meier Survival Curves Entry to Marriage by Women's Education and Cohort



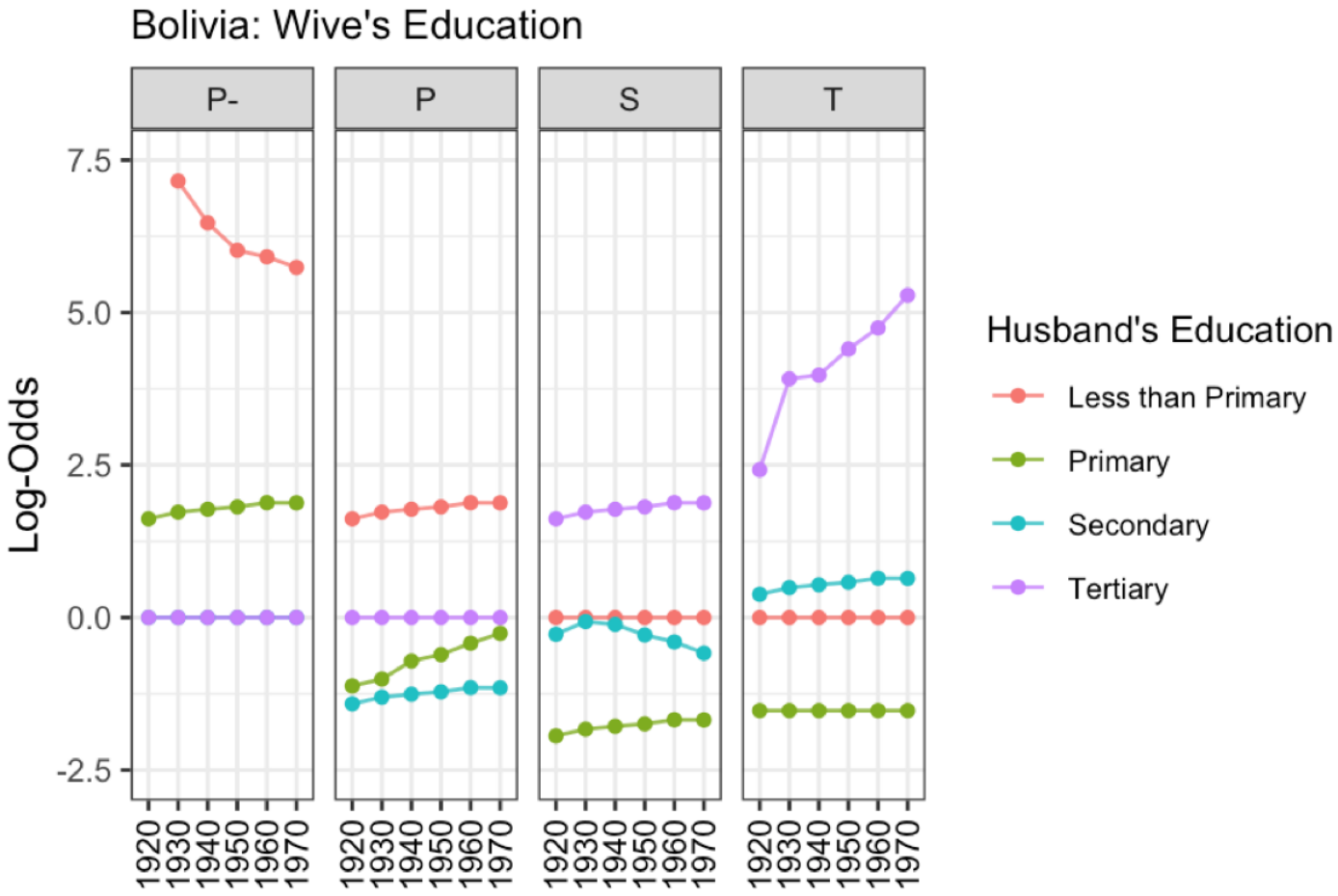
Note: Figures created by the author using DHS data (IPUMS 2020).

Figure 4: Cohort Trends Marital Pairings: All Unions



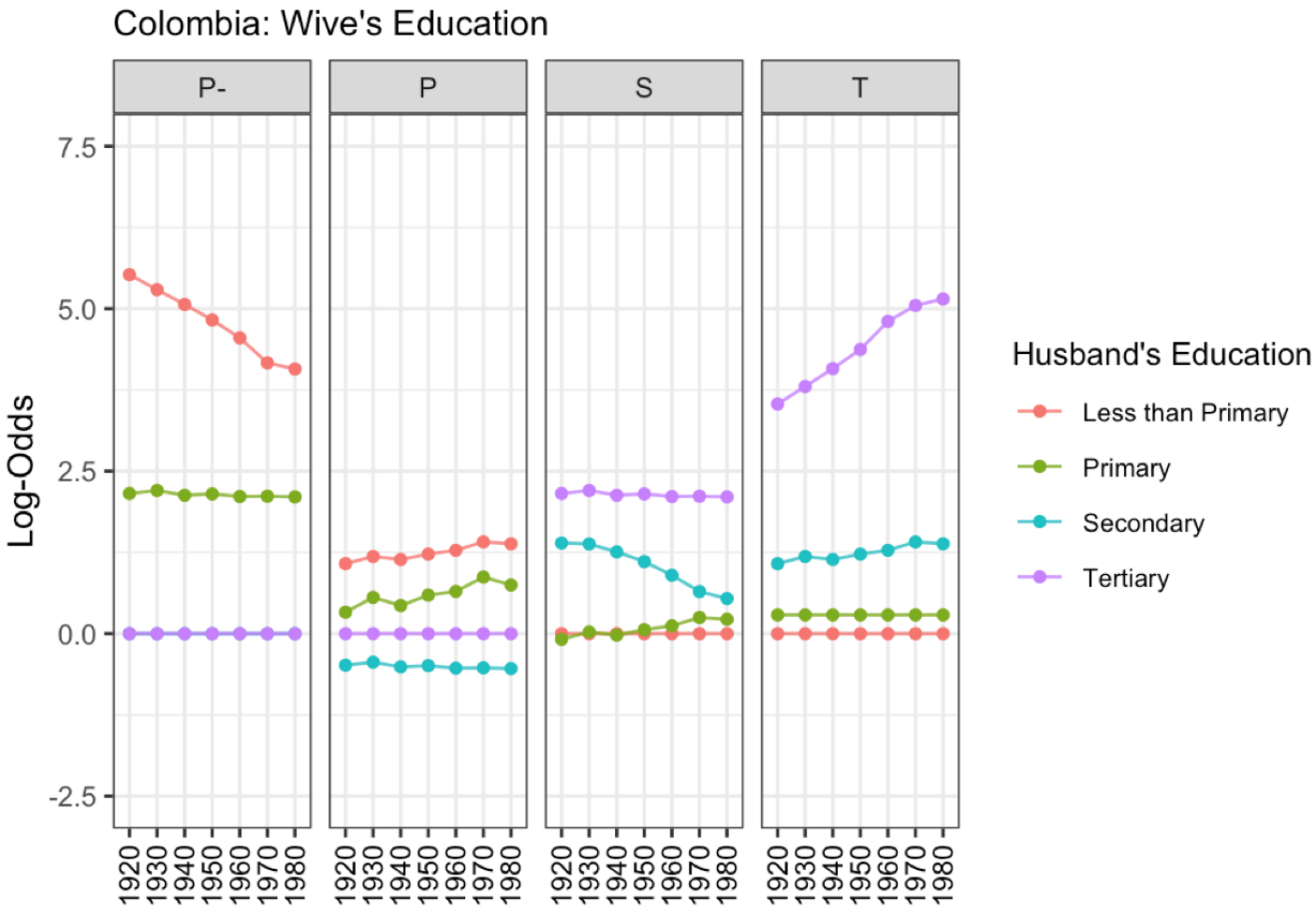
Note: Figures created by the author using Census data (IPUMS 2020).

Figure 5: Educational Assortative Mating Across Women's Birth Cohorts, Preferred Log-linear Model, Bolivia DHS.



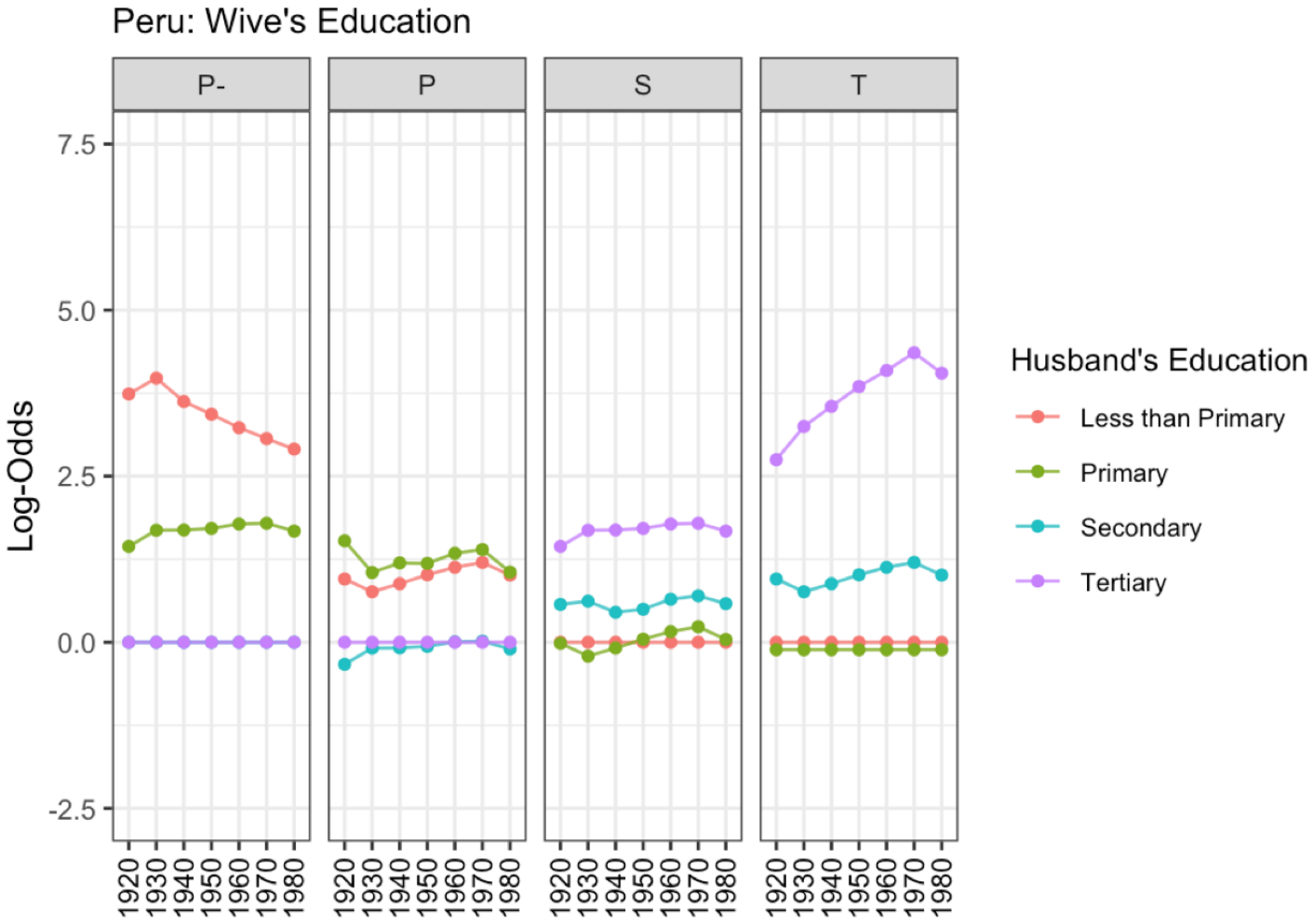
Note: Parameters from log-linear model CI + [Diag][DiagY][Sym][SymY]. Log odds were calculated from fitted frequencies.

Figure 6: Educational Assortative Mating Across Women's Birth Cohorts, Preferred Log-linear Model, Colombia DHS.



Note: Parameters from log-linear model CI + [Diag][DiagY][Asym][AsymY]. Log odds were calculated from fitted frequencies.

Figure 7: Educational Assortative Mating Across Women's Birth Cohorts, Preferred Log-linear Model, Peru DHS.



Note: Parameters from log-linear model CI + [Diag][DiagY][Asym][AsymY]. Log odds were calculated from fitted frequencies.

Tables

Table 1: Analytical Sample DHS (Women Only)

	Bolivia			Colombia			Peru		
	Mean	**SD**	**N**	**Mean**	**SD**	**N**	**Mean**	**SD**	**N**
Years of education	8.18	5.17	27228	8.78	4.36	109949	9.04	4.65	143336
Mean age at survey	32.58	8.44	27229	33.44	8.63	109949	33.51	8.47	143336
Married/Cohabitating	0.73	0.45	27229	0.63	0.48	109949	0.70	0.46	143336
Never Married	0.17	0.38	27229	0.19	0.39	109949	0.18	0.39	143336
Age first marriage	20.04	4.51	22522	20.04	4.82	89553	20.14	4.61	117085
Head of household	0.14	0.35	27228	0.20	0.40	109948	0.13	0.33	143336
Wife	0.61	0.49	27228	0.49	0.50	109948	0.57	0.50	143336
Daughter	0.18	0.39	27228	0.22	0.41	109948	0.22	0.42	143336
Rural background	0.32	0.47	27027	0.36	0.48	23978	0.36	0.48	143106
Minority	0.36	0.48	27206	0.21	0.41	69583	0.12	0.32	143326
Currently working	0.73	0.44	27219	0.59	0.49	109948	0.80	0.40	143336

Table 2: Descriptive Statistics by Cohort

Proportion Never Married at Age 35				
	Bolivia	***Colombia***	***Peru***	Data
1950	9.6	15.1	12.4	Census
1960	13.9	17.4	13.1	Census
1970	21.1	18.3	14.9	Census
1980	N/A	N/A	N/A	Census
Mean Age at Marriage				
	Bolivia	***Colombia***	***Peru***	Data
1950	21.1	21.0	21.0	DHS
1960	20.8	21.2	20.9	DHS
1970	20.1	20.2	20.4	DHS
1980	18.4	18.8	18.9	DHS
Mean Years of Education				
	Bolivia	***Colombia***	***Peru***	Data
1950	5.67	6.55	6.87	DHS
1960	6.77	7.6	7.78	DHS
1970	8.4	8.6	9	DHS
1980	9.9	9.8	10.1	DHS

Table 3: Discrete Logit Model: Baseline

	Bolivia	Colombia	Peru
Exposure (15-18)			
Age 19-24	1.13***	0.67***	0.90***
	(0.04)	(0.03)	(0.02)
Age 25-29	0.60***	-0.12**	0.21***
	(0.06)	(0.06)	(0.03)
Age 30+	0.10	0.40***	-0.29***
	(0.09)	(0.11)	(0.06)
Education (Less than Primary)			
Primary	0.14**	0.17	0.23***
	(0.07)	(0.12)	(0.06)
Secondary	-0.34***	-0.18	-0.10
	(0.07)	(0.12)	(0.07)
Tertiary	-1.18***	-1.13***	-1.14***
	(0.07)	(0.12)	(0.07)
Birth Cohort (1950-60)			
1970-80	-0.03		
	(0.04)		
1975-85		-0.20***	-0.11***
		(0.04)	(0.04)
Controls			
Rural Background	-0.02	0.08*	0.11***

	(0.04)	(0.04)	(0.02)
Ethnicity Aymara	-0.01		
	(0.04)		
Ethnicity Quechua	-0.07		
	(0.05)		
Ethnicity Other	0.37***		
	(0.09)		
Ethnicity Aymara			-0.20***
			(0.05)
Ethnicity Quechua			-0.18*
			(0.11)
Ethnicity Other			0.32***
			(0.11)
Constant	0.21**	-0.30**	-0.07
	(0.11)	(0.14)	(0.09)
Observations	20180	20223	96555

All models control for region and ethnicity. Standard errors are clustered by survey cluster. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4: Discrete Logit Model 2

	Bolivia	Colombia	Peru
Exposure (15-18)			

Age 19-24	0.89***	0.29	0.34***
	(0.13)	(0.25)	(0.12)
Age 25-29	0.37**	0.07	-0.86***
	(0.17)	(0.37)	(0.18)
Age 30+	0.40	0.34	-0.75***
	(0.26)	(0.57)	(0.28)
Education (Less than Primary)			
Primary	0.18**	0.24	0.21***
	(0.08)	(0.16)	(0.07)
Secondary	-0.42***	-0.23	-0.30***
	(0.09)	(0.16)	(0.07)
Tertiary	-1.80***	-1.55***	-1.97***
	(0.11)	(0.16)	(0.08)
Birth Cohort (1950-60)			
1970-1980	-0.05		
	(0.04)		
1975-1985		-0.20***	-0.14***
		(0.04)	(0.03)
Exposure x Education			
Age 19-24 x Primary	0.05	0.09	0.03
	(0.14)	(0.26)	(0.13)
Age 25-29 x Primary	-0.38**	-0.88**	-0.04
	(0.19)	(0.40)	(0.19)

Age 30+ x Primary	-0.90***	-0.62	-0.62**
	(0.30)	(0.61)	(0.31)
Age 19-24 x Secondary	0.32**	0.31	0.46***
	(0.14)	(0.25)	(0.13)
Age 25-29 x Secondary	0.10	-0.46	0.65***
	(0.19)	(0.38)	(0.19)
Age 30+ x Secondary	-0.43	-0.10	-0.09
	(0.31)	(0.59)	(0.30)
Age 19-24 x Tertiary	0.84***	0.80***	1.24***
	(0.15)	(0.26)	(0.13)
Age 25-29 x Tertiary	1.30***	0.68*	2.12***
	(0.20)	(0.38)	(0.19)
Age 30+ x Tertiary	0.76***	1.28**	1.70***
	(0.29)	(0.61)	(0.29)
Controls	Yes	Yes	Yes
Constant	0.31***	-0.20	0.21**
	(0.12)	(0.17)	(0.09)
Observations	20180	20223	96555

All models control for region, rural/urban, and ethnicity. Standard errors are clustered by survey cluster. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 5: Discrete Logit Model 3

	Bolivia	Colombia	Peru
Exposure (15-18)			
Age 19-24	0.90***	0.29	0.34***
	(0.13)	(0.25)	(0.12)
Age 25-29	0.38**	0.08	-0.86***
	(0.17)	(0.37)	(0.18)
Age 30+	0.42	0.36	-0.75***
	(0.26)	(0.58)	(0.28)
Education (Less than Primary)			
Primary	0.09	0.14	-0.01
	(0.11)	(0.18)	(0.12)
Secondary	-0.21	-0.28	-0.42***
	(0.13)	(0.18)	(0.12)
Tertiary	-1.24***	-1.09***	-1.66***
	(0.14)	(0.19)	(0.13)
Birth Cohort (1950-60)			
1970-1980	0.10		
	(0.11)		
1975-1985		-0.15	-0.19*
		(0.24)	(0.11)
Exposure x Education			
Age 19-24 x Primary	0.05	0.10	0.03
	(0.14)	(0.26)	(0.13)

Age 25-29 x Primary	-0.38**	-0.85**	-0.02
	(0.19)	(0.40)	(0.19)
Age 30+ x Primary	-0.88***	-0.52	-0.58*
	(0.30)	(0.62)	(0.32)
Age 19-24 x Secondary	0.32**	0.31	0.46***
	(0.14)	(0.25)	(0.13)
Age 25-29 x Secondary	0.09	-0.45	0.65***
	(0.20)	(0.38)	(0.19)
Age 30+ x Secondary	-0.46	-0.05	-0.08
	(0.32)	(0.60)	(0.30)
Age 19-24 x Tertiary	0.86***	0.81***	1.25***
	(0.15)	(0.26)	(0.13)
Age 25-29 x Tertiary	1.31***	0.62	2.12***
	(0.20)	(0.39)	(0.19)
Age 30+ x Tertiary	0.73**	0.82	1.68***
	(0.30)	(0.62)	(0.30)
Education x Cohort			
Primary x 1970-1980	0.05		
	(0.12)		
Secondary x 1970-1980	-0.30**		
	(0.14)		
Tertiary x 1970-1980	-0.72***		

	(0.14)		
Primary x 1975-1985		0.19	0.25**
		(0.26)	(0.12)
Secondary x 1975-1985		0.04	0.13
		(0.25)	(0.12)
Tertiary x 1975-1985		-0.58**	-0.33**
		(0.25)	(0.13)
Controls	Yes	Yes	Yes
Constant	0.24*	-0.22	0.25**
	(0.13)	(0.19)	(0.12)
Observations	20180	20223	96555

All models control for region and ethnicity. Standard errors are clustered by survey cluster. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 6: Discrete Logit Model 4

	Bolivia	Colombia	Peru
Exposure (15-18)			
Age 19-24	0.96***	0.57***	0.65***
	(0.14)	(0.12)	(0.14)
Age 25-29	0.59***	-0.17	-0.23
	(0.18)	(0.18)	(0.20)

Age 30+	1.04***	0.10	0.38
	(0.28)	(0.25)	(0.31)
Education (Less than Primary)			
Primary	0.07	0.10	-0.07
	(0.11)	(0.10)	(0.13)
Secondary	-0.25*	-0.40***	-0.58***
	(0.14)	(0.11)	(0.13)
Tertiary	-1.34***	-1.43***	-2.09***
	(0.15)	(0.12)	(0.15)
Birth Cohort (1950-60)			
1970-1980	0.26**		
	(0.11)		
1975-1985		0.29	0.12
		(0.11)	(0.12)
Exposure x Education			
Age 19-24 x Primary	0.07	0.25**	0.12
	(0.14)	(0.12)	(0.13)
Age 25-29 x Primary	-0.24	-0.06	0.16
	(0.19)	(0.19)	(0.20)
Age 30+ x Primary	-0.55*	-0.13	-0.22
	(0.32)	(0.26)	(0.33)
Age 19-24 x Secondary	0.35**	0.64***	0.58***
	(0.14)	(0.12)	(0.13)

Age 25-29 x Secondary	0.29	0.58***	0.92***
	(0.20)	(0.18)	(0.19)
Age 30+ x Secondary	0.14	0.40	0.56*
	(0.33)	(0.26)	(0.31)
Age 19-24 x Tertiary	0.87***	1.19***	1.36***
	(0.16)	(0.12)	(0.13)
Age 25-29 x Tertiary	1.50***	1.69***	2.40***
	(0.21)	(0.18)	(0.19)
Age 30+ x Tertiary	1.32***	1.56***	2.40***
	(0.33)	(0.26)	(0.31)
Education x Cohort			
Primary x 1975-1985	0.03	0.10	0.25*
	(0.12)	(0.11)	(0.13)
Secondary x 1975-1985	-0.30	0.04	0.20
	(0.15)	(0.11)	(0.14)
Tertiary x 1975-1985	-0.64	-0.11	0.005
	(0.15)	(0.12)	(0.15)
Exposure x Cohort			
Age 19-24 x 1975-1985	-0.10	-0.60***	-0.45***
	(0.09)	(0.05)	(0.09)
Age 25-29 x 1975-1985	-0.46***	-0.60***	-0.96***
	(0.13)	(0.07)	(0.12)
Age 30+ x 1975-1985	-1.50***	-1.37***	-2.13***

	(0.19)	(0.10)	(0.16)
Constant	0.15	-0.28***	0.09
	(0.13)	(0.11)	(0.13)
Observations	20180	20223	96555

All models control for region and ethnicity. Standard errors are clustered by survey cluster. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 7: Discrete Logit Model 5

	Bolivia	Colombia	Peru
Exposure (15-18)			
Age 19-24	0.91***	0.39**	0.78***
	(0.18)	(0.19)	(0.23)
Age 25-29	0.69***	-0.64**	0.12
	(0.24)	(0.26)	(0.35)
Age 30+	0.99***	-0.21	0.80
	(0.34)	(0.29)	(0.49)
Education (Less than Primary)			
Primary	0.03	-0.01	0.05
	(0.13)	(0.12)	(0.16)
Secondary	-0.23	-0.62***	-0.57***
	(0.17)	(0.12)	(0.17)
Tertiary	-1.29***	-1.68***	-1.79***

	(0.20)	(0.16)	(0.21)
Birth Cohort (1950-60)			
1970-1980	0.25*		
	(0.14)		
1975-1985		0.08	0.25
		(0.14)	(0.16)
Exposure x Education			
Age 19-24 x Primary	0.20	0.34*	-0.23
	(0.21)	(0.20)	(0.27)
Age 25-29 x Primary	-0.33	0.37	0.01
	(0.29)	(0.29)	(0.40)
Age 30+ x Primary	-0.43	0.19	-0.53
	(0.43)	(0.32)	(0.57)
Age 19-24 x Secondary	0.19	0.89***	0.63**
	(0.26)	(0.21)	(0.28)
Age 25-29 x Secondary	0.14	1.19***	0.73*
	(0.37)	(0.28)	(0.41)
Age 30+ x Secondary	0.72	0.84***	0.19
	(0.57)	(0.32)	(0.57)
Age 19-24 x Tertiary	0.90***	1.51***	1.13***
	(0.27)	(0.24)	(0.30)
Age 25-29 x Tertiary	1.30***	2.21***	1.53***
	(0.36)	(0.31)	(0.42)

Age 30+ x Tertiary	0.88*	1.86***	1.62***
	(0.48)	(0.35)	(0.56)
Education x Cohort			
Primary x 1975-1985	0.07	0.29**	0.08
	(0.16)	(0.15)	(0.18)
Secondary x 1975-1985	-0.34*	0.32**	0.15
	(0.20)	(0.15)	(0.19)
Tertiary x 1975-1985	-0.70***	0.17	-0.35
	(0.23)	(0.18)	(0.23)
Exposure x Cohort			
Age 19-24 x 1975-1985	-0.004	-0.31	-0.63**
	(0.24)	(0.24)	(0.27)
Age 25-29 x 1975-1985	-0.69*	0.27	-1.51***
	(0.36)	(0.36)	(0.42)
Age 30+ x 1975-1985	-1.40***	-0.49	-3.06***
	(0.54)	(0.50)	(0.67)
Exposure x Education x Cohort			
Age 19-24 x Primary x 1975-1985	-0.20	-0.17	0.44
	(0.27)	(0.26)	(0.30)
Age 25-29 x Primary x 1975-1985	0.23	-0.82**	0.31
	(0.41)	(0.38)	(0.47)
Age 30+ x Primary x 1975-1985	-0.21	-0.93*	0.74
	(0.63)	(0.53)	(0.74)

Age 19-24 x Secondary x 1975-1985	0.13	-0.38	-0.01
	(0.32)	(0.26)	(0.31)
Age 25-29 x Secondary x 1975-1985	0.28	-1.03***	0.38
	(0.47)	(0.38)	(0.47)
Age 30+ x Secondary x 1975-1985	-0.77	-1.04**	0.89
	(0.72)	(0.52)	(0.73)
Age 19-24 x Tertiary x 1975-1985	-0.08	-0.45	0.30
	(0.32)	(0.29)	(0.33)
Age 25-29 x Tertiary x 1975-1985	0.35	-0.91**	1.10**
	(0.46)	(0.40)	(0.48)
Age 30+ x Tertiary x 1975-1985	0.49	-0.85	1.34*
	(0.67)	(0.54)	(0.72)
Constant	0.16	-0.10	0.02
	(0.14)	(0.12)	(0.15)
Observations	20180	20223	96555

All models control for region and ethnicity. Standard errors are clustered by survey cluster. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 8: Hazard Predictions Full Discrete Time Model 5

		Bolivia		Colombia		Peru	
Education	**Exposure**	**1950**	**1970**	**1950**	**1970**	**1950**	**1970**
Primary	15-18	0.39	0.47	0.46	0.50	0.46	0.51
Primary	19-24	0.66	0.68	0.64	0.63	0.59	0.66
Primary	25-29	0.48	0.44	0.39	0.41	0.49	0.40
Primary	30+	0.53	0.23	0.45	0.31	0.53	0.22
Secondary	15-18	0.33	0.31	0.32	0.38	0.31	0.39
Secondary	19-24	0.74	0.61	0.65	0.61	0.69	0.62
Secondary	25-29	0.69	0.41	0.47	0.40	0.55	0.42
Secondary	30+	0.73	0.33	0.47	0.29	0.55	0.26
Tertiary	15-18	0.15	0.10	0.14	0.17	0.12	0.12
Tertiary	19-24	0.51	0.38	0.53	0.44	0.47	0.42
Tertiary	25-29	0.55	0.36	0.44	0.39	0.41	0.38
Tertiary	30+	0.53	0.22	0.46	0.31	0.60	0.28

Table 9: Log-Linear Models Association Between Husband's and Wife's Educational Attainment by Cohort: Bolivia.

	df	G2	100*D	AIC	BIC
Indep = [W][H][N]	82.00	160706.73	34.52	160542.73	160840.96
CI = [W][H][N][HW][WY][HN]	43.00	1138.61	1.87	1052.61	1748.75
CI+[CDiag][CDiagN]	38.00	498.20	0.86	422.20	1169.35
CI+[Diag][DiagN]	23.00	37.38	0.12	-8.62	891.57
CI+[Hyper][HyperN]	38.00	685.90	1.04	609.90	1357.05
CI+[Hypo][HypoN]	38.00	444.73	0.75	368.73	1115.88
CI+[CDiag][CDiagN][Sym][SymN]	33.00	459.68	0.77	393.68	1191.85
CI+[CDiag][CDiagN][Asym][AsymN]	28.00	430.86	0.71	374.86	1224.04
CI+[Diag][DiagN][Sym][SymN]	18.00	26.98	0.11	-9.02	942.19
CI+[Diag][DiagN][Asym][AsymN]	13.00	15.29	0.07	-10.71	991.51

Table 10: Log-Linear Models Association Between Husband's and Wife's Educational Attainment by Cohort: Colombia.

	df	G2	100*D	AIC	BIC
Indep = [W][H][N]	99.00	952210.33	32.03	952012.33	952380.30
CI = [W][H][N][HW][WY][HN]	54.00	8388.37	2.61	8280.37	9195.70
CI+[CDiag][CDiagN]	48.00	3282.69	0.92	3186.69	4175.01
CI+[Diag][DiagN]	30.00	514.53	0.28	454.53	1661.80
CI+[Hyper][HyperN]	48.00	4322.78	1.19	4226.78	5215.09
CI+[Hypo][HypoN]	48.00	3080.82	0.89	2984.82	3973.13
CI+[CDiag][CDiagN][Sym][SymN]	42.00	3254.12	0.91	3170.12	4231.41
CI+[CDiag][CDiagN][Asym][AsymN]	36.00	3025.08	0.88	2953.08	4087.36
CI+[Diag][DiagN][Sym][SymN]	24.00	463.01	0.27	415.01	1695.26
CI+[Diag][DiagN][Asym][AsymN]	18.00	173.11	0.10	137.11	1490.34

Table 11: Log-Linear Models Association Between Husband's and Wife's Educational Attainment by Cohort: Peru.

	df	G2	100*D	AIC	BIC
Indep = [W][H][N]	98.00	437880.74	34.09	437684.74	438040.59
CI = [W][H][N][HW][WY][HN]	53.00	1842.84	1.61	1736.84	2602.12
CI+[CDiag][CDiagN]	47.00	1456.04	1.43	1362.04	2295.24
CI+[Diag][DiagN]	29.00	219.38	0.27	161.38	1298.35
CI+[Hyper][HyperN]	47.00	1624.18	1.49	1530.18	2463.39
CI+[Hypo][HypoN]	47.00	1382.53	1.39	1288.53	2221.73
CI+[CDiag][CDiagN][Sym][SymN]	41.00	1397.88	1.37	1315.88	2317.00
CI+[CDiag][CDiagN][Asym][AsymN]	35.00	1320.42	1.36	1250.42	2319.47
CI+[Diag][DiagN][Sym][SymN]	23.00	172.16	0.25	126.16	1331.06
CI+[Diag][DiagN][Asym][AsymN]	17.00	82.73	0.15	48.73	1321.55

Table 12: Preferred Log-Linear Model by Country

	Bolivia	Colombia	Peru
Cohort 1930	1.88***	1.17***	3.05***
	(0.20)	(0.08)	(0.30)
Cohort 1940	3.01***	1.88***	4.29***
	(0.18)	(0.07)	(0.30)
Cohort 1950	3.35***	2.15***	4.89***
	(0.18)	(0.07)	(0.30)
Cohort 1960	2.87***	2.10***	5.03***
	(0.18)	(0.07)	(0.30)
Cohort 1970	2.21***	1.69***	4.40***
	(0.18)	(0.07)	(0.30)
Cohort 1980		0.49***	3.30***
		(0.07)	(0.30)
Women Primary	2.38***	2.39***	-0.57
	(0.16)	(0.07)	(0.38)
Women Secondary	0.84***	-0.01	-0.40
	(0.17)	(0.06)	(0.29)
Women Tertiary	-2.84***	-2.57***	-2.44***
	(0.52)	(0.12)	(0.52)
Men Primary	4.37***	1.53***	0.35
	(0.19)	(0.07)	(0.43)

Men Secondary	3.58***	0.26***	1.19***
	(0.15)	(0.07)	(0.30)
Men Tertiary	1.02***	-0.97***	-0.49
	(0.17)	(0.08)	(0.36)
Homogamy Less Primary	8.30***	5.52***	3.74***
	(0.19)	(0.07)	(0.30)
Homogamy Primary	-1.12***	0.33***	1.53**
	(0.26)	(0.09)	(0.64)
Homogamy Secondary	-0.27	1.39***	0.57*
	(0.23)	(0.09)	(0.34)
Homogamy Tertiary	2.42***	3.53***	2.75***
	(0.78)	(0.15)	(0.66)
Symmetrical	1.62***		
	(0.14)		
Assymetrical 1		1.08***	0.95**
		(0.08)	(0.41)
Assymetrical 2		2.16***	1.44***
		(0.05)	(0.30)
Cohort 1930 x Women Primary	-0.32*	-0.17**	0.66*
	(0.17)	(0.08)	(0.39)
Cohort 1940 x Women Primary	-0.47***	-0.07	0.37
	(0.15)	(0.07)	(0.38)

Cohort 1950 x Women Primary	-0.31**	0.08	0.63*
	(0.15)	(0.07)	(0.38)
Cohort 1960 x Women Primary	0.15	0.20***	0.84**
	(0.15)	(0.07)	(0.38)
Cohort 1970 x Women Primary	0.25	-0.09	1.22***
	(0.15)	(0.07)	(0.38)
Cohort 1980 x Women Primary		0.20**	1.83***
		(0.08)	(0.38)
Cohort 1930 x Women Secondary	-0.31*	0.18***	0.26
	(0.17)	(0.07)	(0.29)
Cohort 1940 x Women Secondary	-0.08	0.61***	0.49*
	(0.16)	(0.06)	(0.29)
Cohort 1950 x Women Secondary	0.49***	1.26***	0.89***
	(0.15)	(0.06)	(0.29)
Cohort 1960 x Women Secondary	1.18***	1.71***	1.04***
	(0.15)	(0.06)	(0.29)
Cohort 1970 x Women Secondary	1.51***	1.87***	1.36***
	(0.15)	(0.06)	(0.29)
Cohort 1980 x Women Secondary		2.25***	1.86***
		(0.07)	(0.29)
Cohort 1930 x Women Tertiary	0.17	0.06	0.01
	(0.51)	(0.13)	(0.53)

Cohort 1940 x Women Tertiary	0.43	0.87***	0.37
	(0.48)	(0.12)	(0.53)
Cohort 1950 x Women Tertiary	0.95**	1.55***	0.56
	(0.47)	(0.12)	(0.53)
Cohort 1960 x Women Tertiary	1.54***	1.62***	0.55
	(0.47)	(0.12)	(0.52)
Cohort 1970 x Women Tertiary	1.35***	1.77***	0.80
	(0.48)	(0.12)	(0.53)
Cohort 1980 x Women Tertiary		1.04***	0.57
		(0.13)	(0.53)
Cohort 1930 x Men Primary	-0.65***	-0.07	0.25
	(0.20)	(0.07)	(0.44)
Cohort 1940 x Men Primary	-0.84***	-0.04	0.01
	(0.19)	(0.06)	(0.43)
Cohort 1950 x Men Primary	-0.84***	-0.11*	0.13
	(0.18)	(0.06)	(0.43)
Cohort 1960 x Men Primary	-0.48***	-0.07	0.25
	(0.18)	(0.06)	(0.43)
Cohort 1970 x Men Primary	-0.40**	-0.30***	0.41
	(0.19)	(0.06)	(0.43)
Cohort 1980 x Men Primary		-0.29***	0.80*
		(0.07)	(0.43)

Cohort 1930 x Men Secondary	-0.61***	0.35***	0.10
	(0.14)	(0.07)	(0.30)
Cohort 1940 x Men Secondary	-0.50***	0.54***	0.38
	(0.13)	(0.07)	(0.30)
Cohort 1950 x Men Secondary	-0.31**	0.82***	0.63**
	(0.13)	(0.06)	(0.30)

Table 13: Continued: Preferred Log-Linear Model by Country

	Bolivia	Colombia	Peru
Cohort 1960 x Men Secondary	0.07	0.98***	0.75**
	(0.13)	(0.06)	(0.30)
Cohort 1970 x Men Secondary	0.33**	0.95***	0.85***
	(0.13)	(0.06)	(0.30)
Cohort 1980 x Men Secondary		1.05***	0.98***
		(0.07)	(0.30)
Cohort 1930 x Men Tertiary	-0.38**	0.08	0.01
	(0.19)	(0.08)	(0.36)
Cohort 1940 x Men Tertiary	-0.48***	0.04	0.13
	(0.18)	(0.08)	(0.36)
Cohort 1950 x Men Tertiary	-0.68***	-0.23***	0.12

	(0.17)	(0.08)	(0.36)
Cohort 1960 x Men Tertiary	-0.87***	-0.79***	-0.09
	(0.17)	(0.08)	(0.36)
Cohort 1970 x Men Tertiary	-1.29***	-1.03***	-0.24
	(0.18)	(0.08)	(0.36)
Cohort 1980 x Men Tertiary		-1.33***	-0.46
		(0.09)	(0.36)
Women Primary x Men Primary			
Women Secondary x Men Primary	-3.56***	-1.16***	-0.97***
	(0.08)	(0.06)	(0.06)
Women Tertiary x Men Primary	-1.53***	0.29***	-0.11***
	(0.24)	(0.02)	(0.04)
Women Primary x Men Secondary	-3.03***	-2.64***	-1.77***
	(0.08)	(0.05)	(0.04)
Women Secondary x Men Secondary			
Women Tertiary x Men Secondary	-1.24***		
	(0.23)		
Cohort 1930 x Homogamy LP	-1.14***	-0.23***	0.24
	(0.20)	(0.08)	(0.31)
Cohort 1940 x Homogamy LP	-1.83***	-0.46***	-0.12

	(0.18)	(0.07)	(0.30)
Cohort 1950 x Homogamy LP	-2.28***	-0.70***	-0.31
	(0.18)	(0.07)	(0.30)
Cohort 1960 x Homogamy LP	-2.38***	-0.97***	-0.51*
	(0.18)	(0.07)	(0.30)
Cohort 1970 x Homogamy LP	-2.56***	-1.36***	-0.67**
	(0.18)	(0.07)	(0.30)
Cohort 1980 x Homogamy LP		-1.45***	-0.83***
		(0.08)	(0.30)
Cohort 1930 x Homogamy P	0.11	0.23**	-0.48
	(0.28)	(0.10)	(0.65)
Cohort 1940 x Homogamy P	0.41	0.10	-0.33
	(0.26)	(0.09)	(0.65)
Cohort 1950 x Homogamy P	0.52**	0.26***	-0.34
	(0.26)	(0.09)	(0.64)
Cohort 1960 x Homogamy P	0.70***	0.32***	-0.19
	(0.26)	(0.09)	(0.64)
Cohort 1970 x Homogamy P	0.86***	0.54***	-0.13
	(0.26)	(0.09)	(0.64)
Cohort 1980 x Homogamy P		0.42***	-0.47
		(0.10)	(0.65)
Cohort 1930 x Homogamy S	0.21	-0.01	0.05

	(0.24)	(0.09)	(0.35)
Cohort 1940 x Homogamy S	0.16	-0.14	-0.12
	(0.22)	(0.09)	(0.34)
Cohort 1950 x Homogamy S	-0.01	-0.29***	-0.07
	(0.22)	(0.08)	(0.34)
Cohort 1960 x Homogamy S	-0.12	-0.50***	0.08
	(0.22)	(0.08)	(0.34)
Cohort 1970 x Homogamy S	-0.31	-0.75***	0.13
	(0.22)	(0.08)	(0.34)
Cohort 1980 x Homogamy S		-0.85***	0.01
		(0.09)	(0.34)
Cohort 1930 x Homogamy PS	1.49*	0.27	0.50
	(0.78)	(0.17)	(0.67)
Cohort 1940 x Homogamy PS	1.55**	0.54***	0.80
	(0.76)	(0.15)	(0.67)
Cohort 1950 x Homogamy PS	1.98***	0.84***	1.10*
	(0.75)	(0.15)	(0.66)
Cohort 1960 x Homogamy PS	2.33***	1.27***	1.34**
	(0.75)	(0.15)	(0.66)
Cohort 1970 x Homogamy PS	2.86***	1.52***	1.61**
	(0.75)	(0.15)	(0.66)
Cohort 1930 x Symmetrical 1	0.11		

	(0.16)		
Cohort 1940 x Symmetrical 1	0.16		
	(0.15)		
Cohort 1950 x Symmetrical 1	0.20		
	(0.15)		
Cohort 1960 x Symmetrical 1	0.26*		
	(0.15)		
Cohort 1970 x Symmetrical 1	0.26*		
	(0.15)		
Cohort 1980 x Homogamy PS		1.62***	1.30*
		(0.16)	(0.67)
Cohort 1930 x Asymmetrical 1		0.11	-0.19
		(0.08)	(0.42)
Cohort 1940 x Asymmetrical 1		0.06	-0.07
		(0.08)	(0.41)
Cohort 1950 x Asymmetrical 1		0.15**	0.06
		(0.07)	(0.41)
Cohort 1960 x Asymmetrical 1		0.21***	0.18
		(0.07)	(0.41)
Cohort 1970 x Asymmetrical 1		0.33***	0.25
		(0.07)	(0.41)
Cohort 1980 x Asymmetrical 1		0.31***	0.06

		(0.08)	(0.41)
Cohort 1930 x Asymmetrical 2		0.05	0.24
		(0.05)	(0.31)
Cohort 1940 x Asymmetrical 2		-0.03	0.25
		(0.05)	(0.30)
Cohort 1950 x Asymmetrical 2		-0.01	0.27
		(0.05)	(0.30)
Cohort 1960 x Asymmetrical 2		-0.05	0.34
		(0.05)	(0.30)
Cohort 1970 x Asymmetrical 2		-0.04	0.35
		(0.05)	(0.30)
Cohort 1980 x Asymmetrical 2		-0.05	0.23
		(0.06)	(0.30)
Constant	0.49***	4.67***	2.38***
	(0.19)	(0.07)	(0.30)
Observations	94	112	111
Log Likelihood	-374.28	-628.11	-531.37
AIC	900.56	1,444.23	1,250.74
Note:			*p<0.1; **p<0.05; ***p<0.01

Chapter 4

Mass Education and Women's Autonomy: Evidence from Latin America

Introduction

In the last few decades, compulsory school reforms have been implemented across most countries in the Global South (Buchmann and Hannum 2001). A direct consequence of these policies has been a dramatic rise in women's schooling and a reduction of the gender gap in educational attainment (Dorius and Firebaugh 2010; Grant and Behrman 2010). Scholars have investigated the effects of further schooling on women's marriage timing, child mortality, and intimate partner violence, among other outcomes (Andriano and Monden 2019; Grant 2015; Weitzman 2017). However, less is known about how mass education causally impacts female autonomy, an important predictor of women's health, reproductive behavior, and overall well-being (Balk 1994; Dyson and Moore 1983; Oppenheimer 1997; Upadhyay and Hindin 2005).

Demographers and development scholars have argued that mass schooling accelerates population transformations by increasing women's status in the home and enabling them to decide about their health, earnings, and movement (Caldwell 1980; Inkeles 1969; Therborn 2004). Similarly, aid agencies and multilateral organizations have embraced mass schooling as an effective policy for increasing women's autonomy in high gender inequality countries (Murphy-Graham

and Lloyd 2016). However, gains in women's schooling may have unexpected implications for female autonomy in contexts where hypergamy norms—the ideal that men should marry down and women should marry up in education and other markers of social status—are still dominant. The prevalence of these expectations has been associated with marriage penalties for educated women and high tensions among couples that do not conform to this ideal—including higher rates of intimate partner violence (Behrman 2019; Jewkes 2002; Vyas and Watts 2009). Thus, if women's gains in education represent a challenge to men's dominant status in families, mass schooling may unexpectedly hinder women's autonomy in these contexts.

Existent scholarship in the Global South is inconclusive about the effect of mass schooling on women's autonomy. While some studies show that education is associated with women's greater autonomy (Acharya et al. 2010; Basu 1992; Jejeebhoy and Sathar 2001; Oropesa 1997), others find its ineffective (Heaton et al. 2005; Malhotra and Mather 1997; Wolf 1985). The heterogeneity of these findings may be due to the endogeneity of schooling. As women's education and autonomy are both predicted by family background, cognitive ability, and other unobserved differences, establishing a causal effect on the outcome of interest is especially difficult. Additionally, past scholarship has mostly centered on the case of *married* women, as changes in female empowerment tend to occur in relation to husbands. Focusing on this sub-population, however, also introduces selection biases given the correlation between education and union formation patterns (Schwartz and Mare 2005; Raymo 2003).

This chapter addresses these challenges by investigating three questions. First, what is the effect of mass schooling on women's autonomy? Second, does the impact of schooling vary according to women's background? Third, are changes in women's union formation a potential pathway through which education affects their autonomy? I answer these questions by leveraging quasi-experimental variation in compulsory schooling reforms enacted in Bolivia, Colombia, and Peru. These Latin American countries implemented these reforms during the 1990s, at similar stages of development and amidst the prevalence of hypergamy norms in families, especially among rural populations (Ingoldsby 1991; García and de Oliveira 1994). However, these societies

differed on their level of gender inequality in key domains. For instance, while in Bolivia and Peru, women were still catching-up to men's schooling, Colombia already presented a female advantage in primary and secondary education at the time of these reforms (IPUMS 2020). Thus, this comparison allows me to examine the effects of mass schooling on women's autonomy across contexts that share similar socio-economic and cultural conditions, but that vary in their degree of gender inequality.

In this analysis, I first assess the over time changes in the returns to schooling for women's autonomy across birth cohorts differently exposed to educational expansion. I then focus on the effects of compulsory schooling reforms via an instrumental variable design, using random exposure to these laws as an instrument for years of education. This approach allows me to disentangle the effect of schooling on women's autonomy from other unobserved factors that could confound this relationship.

My findings show that there is a positive association between women's schooling and greater autonomy. However, as educational systems expand, these returns progressively diminish in magnitude. Once I address endogeneity via instrumental variables, I find that women who complied with compulsory laws and gained further education experienced a decrease in their autonomy in all countries. Notably, this effect was smaller in magnitude in the case of Colombia. Additional analyses confirm that while for most women education was a means of empowerment, for those who were less likely to continue their education in the absence of compulsory reforms, further schooling did not fulfill the promise of greater autonomy. I show that population heterogeneity and union formation behavior may be important pathways explaining these effects. Together, this study contributes to a growing literature documenting the unexpected implications of rapid increases in women's status in contexts where hypergamy norms and gender inequality are still ubiquitous.

Literature review

Mass Education and Women's Autonomy

The enhancement of women's autonomy and decision-making has long been theorized as one of the benefits of educational expansion. Based on the experience of Western countries, modernization scholars promoted mass schooling in the Global South as an effective tool to diffuse the values and motivations associated with development (Marion 1966; Portes 2015). The latter would include increased independence from parental authority, greater ambition for oneself, and women's emancipation from subordinate roles (Inkeles 1969; Goode 1963). The promotion of "developmental idealism", including the notion that modern nuclear families are the root of development, had a significant influence on the adoption of mass schooling policies in the Global South (Thornton 2001; Thornton et al. 2015).

Demographers also viewed mass schooling as a unique policy to speed up demographic change and socioeconomic prosperity in developing contexts. In his seminal article on education and fertility decline, Caldwell posited that the greatest effect of schooling would be on the transformation of family relationships and norms. In particular, education was expected to increase women's autonomy and decision-making relative to traditional sources of authority in families, namely, parents and husbands. Population scholars argued that women's enhanced autonomy would play an essential role in their demographic behavior, and in the reduction of fertility in particular (Caldwell 1980; Cleland and van Ginneken 1988).

Different mechanisms have been hypothesized to explain the relationship between mass education and increases in women's autonomy. Some argued that education would empower women by exposing them to gender-egalitarian attitudes (Bledsoe et al. 1999; Caldwell 1980), enhancing their cognitive abilities to make informed decisions (Kabeer 2005), and increasing their socioeconomic resources (Jejeebhoy 1995). However, as educational expansion unfolded in the Global South, several limitations of schooling for the enhancement of women's autonomy became appar-

ent. The most alarming issues were the continuous enforcement of gender biases in schools (King and Hill 1993; Ohsako 1997; Stromquist 2001), as well as the persistent low quality of educational institutions (Pritchett 2013; Willms and Somer 2001).

Notably, mass education may also increase women's autonomy via the type of households and partnerships that educated women form in adulthood. To *whom* women get married may matter for their autonomy and decision-making, especially because changes in these domains occur relative to other household members, and most importantly, their husbands (Basu 1992). Because partners may have conflicting interests and preferences, the bargaining position of each spouse shapes who has more agency and decision-making power to pursue those preferences (Agarwal 1997; Kabeer 2007). From a resource-exchange perspective, each spouse's bargaining power is a function of their relative resources, such as income or education, and their ability to effectively leave the marriage if necessary (Cook and Whitmeyer 1992; Lundberg and Pollak 1993). In most western countries, highly educated women tend to marry men who are equally educated as themselves (Blossfeld 2009; Schwartz and Mare 2005), leading to unions with lower intramarital differences, and therefore, more balanced power relations. More educated women also tend to form unions with lower intramarital age gaps (Baird et al. 2010), which tend to be more symmetrical in education and economic standing (Pyke 2010; Malhotra and Mather 1997). Thus, women exposed to compulsory laws may be more likely to enter unions with more egalitarian power relations in which husbands are more prone to share household decisions and foster women's autonomy.

Up to this point, we have assumed that the benefits of education for women's autonomy do not change over time. Yet, the positive effects of schooling may vary as the number of educated women increases across birth cohorts. Recent scholarship in Europe and the U.S has found that as education expands and more individuals obtain an educational credential, the marginal effect of such credential declines (Horowitz 2015). These relative effects of education have been found for labor market outcomes (Bol 2015; Horowitz 2018) and civic participation (Horowitz 2015). Although few studies have investigated how the benefits of schooling on women's autonomy change throughout educational expansion, many scholars have shown that the social meaning of women's

schooling is shaped by their relative position in the educational distribution of their own cohort (Frye and Lopus 2018; Kravdal 2002; Schofer and Meyer 2005). Consequently, the positive effect of schooling on female autonomy may diminish as educational systems expand and higher proportions of women have more schooling.

Women's Education, Hypergamy Norms, and Rural Contexts

Women's education, however, is not always an attractive trait in the marriage market. This is especially the case in contexts of high gender inequality. Hypergamy norms—the expectation that men should marry down and women should marry up in educational attainment, income, and other markers of social status—have historically structured marriage markets in Latin America (Esteve and Florez-Paredes 2018). In particular, the prevalence of educational hypergamy norms has been associated with marriage penalties for educated women, and with higher stress and tensions among couples that deviate from this expectation (Jewkes 2002; Vyas and Watts 2009). For example, educational inconsistencies in marriages have been associated with the limitation of women's autonomy (Koenig et al. 2003), increases in intimate partner violence (Behrman 2019; Oduro et al. 2015; Weitzman 2014), and husband's pursuit of other sexual partners (Hunter 2007; Morrell 2001). Scholars have interpreted these dynamics as an attempt to compensate for or resist the transgression of hypergamy expectations (Goode 1982).

In Latin America, and especially in rural contexts, hypergamy norms are prevalent and expressed in popular discourse as "machismo" (Chant 2002; Giraldo 1972). Beyond the marriage market, this set of gendered beliefs structure families by assigning domestic responsibilities to wives and bread-winning activities to husbands, placing mothers and daughters in a position of inferiority to other males in the home, and encouraging submissive behavior from women while promoting exaggerated masculinity among men (Ingoldsby 1991; Mirande 1997; Orgill and Heaton 2005). Although Latin American women have made important gains in educational attainment, labor participation, and economic independence in recent decades, behaviors consistent with hyper-

gamy ideals still are prevalent (Chant 2002; García and de Oliveira 2011). Across the region, men often resist the desires of wives and daughters to work outside the home (Safa 1995; Psacharopoulos and Tzannatos 1993), downplay women's financial contributions (García and de Oliveira 1994; Kabeer 2007) , and engage in gender-based violence at higher rates than in most other countries (Friedemann-Sánchez and Lovatón 2012). Scholars of Latin America have interpreted these behaviors, including the use of violence, as responses to re-affirm masculine authority when men's dominant status in the home is undermined (Menjivar 2011; Safa 1995).

In this context, and particularly in rural areas, women's schooling may not always be welcome by families, nor attractive for bachelors. Indeed, before compulsory reforms, families in rural regions were more prone to resist women's education. Studies in Peru and Bolivia show that parents tended to favor boys' schooling, and girls often left school to aid with domestic work and farming tasks (Ames 2005; Zapata et al. 2011). In all Latin American countries, more educated women are still less likely to get married, and those that enter a union are more prone to "marry down" in education (Ganguli et al. 2014). These trends may mean that educated women are penalized in the marriage market, given the threatening meaning of their education, and/or that marriage is increasingly unattractive to educated women——a phenomenon also documented in other parts of the world (Raymo 2003).

Together, the interaction between women's schooling and the prevalence of hypergamy norms may shape the effects of education on women's autonomy in unexpected ways. Instead of marrying equally educated men that are more welcoming of women's autonomy, students exposed to these reforms may face difficulties in the marriage market and/or enter status-inconsistent unions, which in turn, may hinder their autonomy and decision-making.

Prior Research and the Present Study

The relationship between women's education and autonomy has received extensive scholarly attention. In developing contexts, several studies have found that more educated women have higher

levels of autonomy and decision-making (Acharya et al. 2010; Basu 1992; Cheng 2019; Jejeebhoy and Sathar 2001). However, other investigations have shown that education does not substantively enhance women's autonomy (Heaton et al. 2005; Malhotra and Mather 1997; Wolf 1985). Quantitative studies in Latin America have found a similar set of findings. In Mexico, Oropesa (1997) shows that education was crucial in increasing wives' decision-making power relative to their husbands. Meanwhile, Heaton et al. (2005) found that schooling moderately increased women's autonomy and decision-making in Bolivia, but had no effects in Peru and Nicaragua.

The heterogeneity of these findings may be due to multiple factors. Because most studies do not address the endogeneity of schooling, it is unclear whether these results are causal. For example, it may be that women with higher schooling are more likely to come from advantaged families, who espouse gender-egalitarian beliefs, and are more prone to enroll their daughters in school. In these situations, we cannot know whether the positive effects of education on autonomy are due to women's schooling or unobserved differences in their family background. Also, prior scholarship has focused on the implications of education for women in marital unions. This seems like a natural choice, as changes in women's empowerment occur relative to other decision-makers in the family, usually their husbands. Nevertheless, because demographers have shown that schooling also shapes women's chances to enter a union and with whom they do so, focusing solely on married women also introduces selection biases.

The present study addresses these two issues by implementing an instrumental variable approach using exposure to compulsory schooling laws in Bolivia, Colombia, and Peru as an instrument. Compulsory laws have often been used in the demographic literature as instruments to uncover the causal effects of schooling (Andriano and Monden 2019; Behrman 2015; Grant 2015; Weitzman 2017). The next section provides further details on the context in which compulsory laws were implemented in Bolivia, Colombia, and Peru.

Background: Compulsory Schooling in Bolivia, Colombia, and Peru

During the 1990s, most Latin American governments implemented compulsory schooling laws. This study focuses on the case of Bolivia, Colombia, and Peru, which all rolled out national compulsory reforms between 1991 and 1994 to raise primary and lower secondary attainments. I study the effects of mass schooling on women's autonomy in these three countries for several reasons. First, the implementation of these reforms significantly increased women's educational attainment in all countries—as I will show in the results—a necessary condition to analyze the impact of greater schooling on female autonomy.

Second, Bolivia, Colombia, and Peru shared several important socioeconomic characteristics—beyond language and the colonial history of Spanish rule—when these reforms were implemented. At that time, these three Andean countries had large rural populations, a medium-high human development index[1], and a GDP per capita below the average in Latin America (See Table A1). In terms of educational indicators, Bolivia and Peru were slightly ahead of Colombia. In 1990, the mean years of education were 6.4 years in Bolivia, 5.5 years in Colombia, and 6.6 years in Peru and less than half of the population older than 25 reached some secondary education across all countries. Moreover, Bolivia, Colombia, and Peru had wide education gaps between urban and rural populations, which were expected to be shortened via compulsory education reforms (UNESCO 2004). In particular, rural populations in Bolivia and Peru tend to be indigenous communities[2], who in the early 90s represented a 55% and 35% of the population, respectively (IPUMS 2020). This was not the case in Colombia[3], where the largest minority group were Afro-descendants—representing a 9.3% of the population—who tended to live in urban areas (IPUMS 2020).

Third, beyond these similarities, these countries also varied in the level of gender inequality

[1] In 1990, the UN defined an index between 0.43 to 0.57 as medium development.
[2] In both countries, Quechua and Aymara were the biggest ethnic groups.
[3] Only a 3.3% of the Colombian population self-identified as indigenous (IPUMS 2020)

when these reforms were implemented. Although Bolivia, Colombia, and Peru shared a common set of gendered beliefs that promote hypergamy norms in the marriage market, men's dominance in families and women's submissive behavior (García and de Oliveira 2011), Colombia was more gender-egalitarian in some key domains relative to Bolivia and Peru. At the time of compulsory reforms, Colombian women had almost equalized men's mean years of education (See Table A1) and those born after 1960 had already surpassed men's educational attainment (IPUMS 2020). In contrast, in Bolivia and Peru, women had almost two years less of education and were significantly less likely to have a secondary education (UNESCO 2020). Drop out rates were especially higher among girls from rural populations, who often left school to help their families with domestic duties (Zapata et al. 2011; Stromquist 2001). Likewise, at similar rates of female labor participation than Bolivia and Peru, Colombia had a smaller gender pay gap (OECD 2016), and household dynamics, such as the division of household labor, were showing signs of change towards more egalitarianism (Friedemann-Sánchez 2006; Kabeer 2007).

Together, comparing these countries offers the opportunity to analyze the effect of compulsory schooling on women's autonomy across contexts with similar socioeconomic and cultural characteristics, but that vary in key aspects of gender inequality.

The Implementation of Compulsory Schooling Reforms

During the 1990s, most Latin American governments implemented compulsory school reforms to increase school enrollment. Table 1 below summarises the characteristics of compulsory schooling reforms in Bolivia, Colombia, and Peru, including their year of implementation and exposed cohorts.

[Table 1 here]

In 1994, Bolivia's newly elected democratic government established that schooling should be extended from five to eight years, making primary education universal and mandatory (UNESCO-

IBE 2006). A central goal of this reform was to increase educational enrollments of indigenous and rural children, which were significantly lower than students in urban areas (UNESCO 2001). To this end, the reform also included the establishment of a multicultural curriculum, creating school materials in multiple languages, and providing teachers with pedagogical support (MINEDUC 2004). Several indicators show that this reform was effective. Between 1994 and 2001, sixth-grade promotion rates increased from 52% to 85% (Contreras and Talavera Simoni 2003), while gross enrollment rates in secondary increased to 73% total (UNESCO 2020).

In Colombia, Law 56 in 1927 declared five years of primary schooling as mandatory, starting at age seven (Ramírez and Téllez 2006; Vargas 2014). In 1991 the new Colombian constitution—enacted to appease the longstanding conflict between the government, paramilitary, and guerrilla groups—lengthened the obligatory educational cycle from five to ten years, including one year of pre-school education (Ramírez and Téllez 2006). Parallel to this reform, the Colombian government also enacted an ambitious school construction plan to enforce the expansion of the educational system. Consequently, between 1993 and 2004, school enrollments for students between 12 and 17 years old increased from 68% to 78% and the urban-rural gap was reduced by eight points (UNESCO 2004).

In the case of Peru, compulsory schooling for students between 6 and 11 years old was established in 1905. In 1993, compulsory schooling was expanded to include ten years of education, extending the minimum leaving age from 11 to 16 years old (UNESCO 2001). Between 1990 and 2010, the Gross Enrollment Ratio for secondary schooling increased a 36% total, suggesting that compulsory reforms effectively attracted new students into the school system (World Bank 2014). Among the population that primarily benefited from this reform were girls and rural children, whose enrollment rates significantly increased after the reform (World Bank 2006). For instance, the percentage of women with some secondary education augmented from being a 38% in 1990 to a 51% in 2000 (UNDP 2020).

Research Design

Data and Sample

For this analysis, I pooled data from the Demographic Health Surveys (DHS) collected between 2000 and 2016 (Heger Boyle et al. 2019).[4] The DHS is a nationally representative household-based survey centered on reproductive and sexual health, children's health, and fertility. Importantly, questionnaires and implementation protocols are standardized to allow for cross-country comparisons. The analytic sample includes all women who were between 20 to 49 years old at the time of the survey.[5] The central analysis compares a sub-sample of women who were just below the average exit age in primary at the time of compulsory reforms (ages 9-11) with respondents just above the primary school exit age at the time of implementation (ages 12-14) (Table A2). In Bolivia, this criterion yields a sample of 3,583 unexposed (born between 1980-1982) and 2,289 exposed women (born between 1983-1985). In Colombia, the sample consists of 11,208 unexposed (born between 1977-1979), and 11,680 exposed (born between 1980-1982) respondents. Lastly, in Peru, the sample comprises 15,585 unexposed (born in 1979-1981) and 16,601 exposed women (born in 1982-1984). Note that Colombia and Peru have much larger sample sizes as the number of DHS surveys available for each country is four and six waves, respectively, while Bolivia only has two surveys available for this analysis.

The analytical sample includes women in different household arrangements. Although most of them are either married or cohabiting, between 30% to 20% percent are single. The majority of the latter lived with their parents (70% pooled across countries) at the time of the survey, while only a smaller percent lived alone (11%) or with other relatives (11%).[6] Given that women's schooling is associated with union formation outcomes, the central analyses of this study make inferences

[4]For Bolivia, I used the 2003 and 2008 DHS, for Colombia, I used the 2000, 2005, 2010, and 2015 DHS, and for Peru, I used the 2004, 2007, 2009, 2010, 2011, 2012 DHS.

[5]I focus solely on women because DHS questions on decision-making and autonomy were answered only by female respondents in all countries.

[6]See Table A2 for descriptive statistics of the analytical sample.

about women's autonomy in the full universe of household configurations. In addition, secondary analyses explore whether women's union formation patterns may be a pathway explaining the effects of schooling on autonomy.

Measures

Outcomes

Following prior demographic literature, this study defines *autonomy* as a woman's ability to execute decisions regarding her personal affairs and those concerning close family members (Bloom et al. 2001; Ghuman 2003; Kabeer 1999). To measure women's autonomy, I use a series of questions[7] about the control over their freedom of movement (item 1), own health (item 2), major household purchases (item 3), and their own money (item 4). Prior studies have validated these items as indicators of women's empowerment in Latin American families (Heaton et al. 2005; Martínez-Restrepo et al. 2017). Note that these questions do not include women's decisions regarding unpaid care work—such as children's nutrition or small household purchases—since in the Latin American context, those decisions are often *expected* of women and do not imply greater autonomy (Folbre 1994; Martínez-Restrepo et al. 2017).

Furthermore, these questions have a series of limitations that are worth mentioning. First, it is unclear whether women's reports about their autonomy actually reflect their behavior. Although I cannot ensure these self-reports indicate what is happening inside women's homes, because these questions present concrete actions and are appropriate for the local context , they still capture relevant information on social norms prescribing women's position in families (Martínez-Restrepo et al. 2017). Second, these measures do not fully capture the multiple dimensions of autonomy (Dyson and Moore 1983). For instance, indicators of women's control over specific reproductive choices are absent from this operationalization. Unfortunately, the need to have comparable measures across countries limited the breadth of questions I could use for this analysis.

[7] See Table A3 for the exact wording of these questions.

Moreover, scholars comparing attitudinal measures across countries have found that even identical questions may not be directly comparable, as reference points and non-extreme responses may be interpreted differently (Cavaille and Marshall 2019). A solution for this is to re-code survey items as dummy variables anchored around reference points that are more likely to be interpreted similarly across countries. Therefore, I transformed each of these questions into binary indicators. I coded each question as "1" if women responded that they took each decision themselves alone, themselves and their partner, or themselves and someone else. Questions were coded "0" if the decision was taken either by the husband or someone else alone. In the case of women who are not in a union, "someone else" generally refers to their parents or other relatives, as only 20% of not-married respondents lived alone or with a non-family member. Moreover, this coding strategy captures whether women have a say, but not absolute dominance over decisions regarding personal and household matters. In that sense, it conceptualizes shared decision-making and negotiated autonomy as indicators of women's empowerment, relative to scenarios where they do not have any say at all on these issues.[8]

In my analysis, I use these binary outcomes both as separate items and as an additive scale. As several scholars have argued, a women's control over one given dimension does not necessarily imply she has similar power over other areas, such as her income or movement (Koenig et al. 2003; Jejeebhoy and Sathar 2001; Malhotra and Mather 1997). In particular, I generate a scale of women's autonomy to capture its multidimensional nature. The autonomy scale is generated by summing the country-specific standardized scores of each re-coded item in Table A3. The resulting scale is sufficiently reliable, having a Cronbach α score higher than 0.6 in all countries (Cortina 1993).[9] Table A4 contains descriptive statistics of autonomy measures by exposure to compulsory reforms.

[8]As a robustness check, I adopted a more "strict" conceptualization of autonomy, achieved only when women are in complete control of their autonomy and decision-making. I coded each item as "1" only if women alone were taking these decisions, and "0" if they were doing so with their husbands, together with someone else, or the decision was taken either by the husband or someone else alone. Results do not change substantively using this alternative coding strategy.

[9]The Cronbach α score was 0.64 in Bolivia, 0.70 in Colombia, and 0.67 in Peru.

Control Variables

Given that I pool different DHS rounds, I include survey year as a categorical variable in all models to account for period effects. I also control for respondents' ethnicity, given the existence of relevant schooling gaps among indigenous and Afro-descendant students. Ethnicity was operationalized as a categorical variable which included "Spanish/White," "Quechua," "Aymara," and "Other Indigenous" for the case of Bolivia and Peru, and "Spanish/White," "Indigenous," "Afro-descendant" and "Other" for Colombia.[10] To control for potential regional differences in school construction, models include the current region of residence under the assumption that respondents have not moved since initiating primary schooling. Given that these regions are broad geographical divisions in all countries, this assumption seems plausible. Results without regional controls are substantively the same, but contain larger standard errors (See Appendix Table A5).

Lastly, to examine heterogeneous treatment effects by women's background, I use a dichotomous version of the ethnicity variable—being indigenous or Afro-descendant versus not– and respondents' residence in childhood. The latter was operationalized as a categorical variable equal to "1" if the respondent lived in a rural area during her childhood and "0" if she lived in a city or town. Since this measure was only available for some survey waves, I could not use it as a pre-treatment measure for region of residence in the main IV models.

Pathways

I explore how union formation and partner characteristics may explain the effect of education on women's autonomy. I first examine respondents' likelihood of being in a marital union measured as a dichotomous indicator equal to "1" if the respondent has never been married or cohabitated and "0" if she has ever been married or cohabitated. The second set of measures focus on partner characteristics among married respondents. In particular, I include (1) a dichotomous indicator for educational homogamy equal to "1" if husbands and wives have equal years of education, (2) a dichotomous indicator for educational hypergamy equal to "1" if husbands have at least one

[10] In the IV analyses ethnicity was not included for Colombia given its high proportion of missing values.

more year of schooling, and (3) a dichotomous indicator for educational hypergamy equal to "1" if husbands have at least three more years of schooling. I also include (4) a dichotomous indicator for educational hypogamy equal to "1" if wives have at least one more year of schooling and (5) a dichotomous indicator for educational hypogamy equal to "1" if wives have at least three more years of schooling. Lastly, I construct(6) a dichotomous indicator for whether the respondent's partner is five years older. To further explore the role of partner characteristics for women's autonomy, I constructed a dummy variable measuring whether husbands engage in controlling behavior. This variable is equal to"1" if women report their husband engaging in any of the following behaviors: (i) being jealous or angry if she talks or talked to other men; (ii) frequently accusing her of being unfaithful; or (iii) trying to limit her contact with her family.

These measures do not distinguish between cohabitation and marriage because of the long tradition of coexistence between these two types of unions in Latin America (Esteve et al. 2012). Indeed, excluding the more economically advanced countries[11], cohabitation remains common at later stages of the life cycle, and non-married couples usually bear children together (Castro Martín 2002; Palloni et al. 1996). In Bolivia, Colombia, and Peru, cohabitation represents almost two-fifths of all marital unions and is not associated with lower levels of education (Esteve et al. 2012). Given the special status of cohabitation in Latin America, recent scholarship considers it has a similar standing as marriage (Castro Martín 2002; Torche 2010). Thus, when using the term marriage in this chapter, I will also be referring to non-marital unions.

Analytical Strategy

To examine the relationship between mass schooling and women's autonomy in adulthood, I conduct two analyses. First, I examine the association between years of education and women's autonomy and how it changes across birth cohorts. The goal of this analysis is to assess how the "autonomy" returns of schooling shift as higher proportions of women access education. To this

[11] Argentina, Chile, and Uruguay.

end, I estimate a series of Ordinary Least Squares (OLS) models where I interact years of schooling with five-year grouped-birth cohorts born between 1970 and 1990. This set of birth cohorts encompasses women exposed to different stages of school expansion, providing a broader view of the relationship between mass education and women's position in the household. In particular, for each country, I estimated the following model:

$$y_{ic} = \beta_0 + \beta_1 Schooling_{ic} + \beta_2 Cohort_{ic} + \beta_3 SchoolxCohort_{ic} + \beta_4 \mathbf{X_{ic}} + \epsilon_{ic} \quad (1)$$

Where $\mathbf{X_i c}$ is a vector of covariates of individual i in country c, including survey year, the region of residence, and ethnicity (in Peru and Bolivia[12]), and u_{ic} are standard errors clustered by survey clusters.

The second analysis estimates the causal effect of women's schooling on their autonomy in the home via an instrumental variable (IV) approach. As mentioned, I compare birth cohorts that were young enough to be exposed to compulsory reforms with cohorts that were barely too old to be impacted by these laws. I use respondents' exposure to these reforms as an instrument for their total years of schooling using a two-stage least squares (2SLS) estimation strategy in each country. Importantly, this approach yields the effect of an extra year of education among respondents that comply with compulsory laws, namely, the Local Average Treatment Effect (LATE) among compliers.

In the first stage, I regress individual i years of completed schooling on a dummy indicator of exposure to compulsory reforms $Exposed_{ic}$ in each country c. Where $\mathbf{X_i c}$, is a vector of covariates for individual i, and u_{ic} are standard errors clustered by survey cluster.

$$Schooling_{ic} = \alpha_0 + \alpha_1 Exposed_{ic} + \alpha_2 \mathbf{X_{ic}} + u_{ic} \quad (2)$$

[12]Colombia only collected ethnicity information on some survey waves, resulting in a high proportion of missing values.

The second stage is a linear probability model where the outcome of interest y_{iac} is regressed on the predicted value of years of completed schooling$_{iac}$ from the first stage. The key parameter is β_1, which yields the treatment effect of one extra year of education on women's autonomy, among respondents that complied with compulsory schooling laws.

$$Outcome_{ic} = \beta_0 + \beta_1 \widehat{Schooling}_{ic} + \beta_2 \mathbf{X_{ic}} + u_{ic} \tag{3}$$

All IV models control for survey year, region, and ethnicity (in Peru and Bolivia), and include robust standard errors, clustered by survey cluster. Two assumptions are needed for this analysis to control for unobserved characteristics and yield causal estimates. First, the instrument must predict treatment but should fulfill the exclusion restriction assumption—affecting the outcome only through the treatment (Angrist and Pischke 2009). Therefore, expansion reforms should affect women's autonomy only via the extra time students spend in the school system, and not through other alternative paths. Later in the chapter, I discuss potential threats to this assumption and explain several robustness checks to address them. The second assumption is that reforms must not reduce the educational attainment of any student—also known as monotonicity. This assumption seems plausible given the net gains in enrollments that all countries experienced after the implementation of compulsory reforms.

To estimate heterogeneous treatment effects according to respondents' residence in childhood, I conduct the same IV procedure for each subgroup separately. Similarly, for the exploration of pathways, I use the same IV approach where instead of estimating the effect of an extra year of schooling on women's autonomy, I assess the effect of education on the likelihood of entering a union and partner characteristics. There are two significant limitations to this supplementary analysis. First, although it can establish a causal link between schooling and each pathway, it cannot do so between each pathway and women's autonomy. Second, this analysis is only exploratory since these pathways are not mutually exclusive, nor do they encompass all possible mechanisms that may explain these results.

Lastly, researchers using instrumental variables typically pay little attention to the characteristics of compliers and non-compliers (Marbach and Hangartner 2020). However, characterizing these groups is an important first step to assess the external validity of the effects estimated by instrumental variables. To this end, I use the approach developed by Marbach and Hangartner (2020), which allows recovering mean values for pre-treatment covariates for compliers and non-compliers. The logic of this procedure is the following. When using IV, researchers can estimate the covariate means of individuals assigned to the control group but still take the treatment—these are the always-takers—and subjects who are assigned to treatment but do not take the treatment—these are the never-takers. Assuming there are no defiers and that the instrument is independently assigned, by subtracting the weighted covariate means of always-takers and never-takers from the entire sample, we can recover the covariate mean for compliers (For more information see Marbach and Hangartner (2020). I implement this procedure in R[13], estimating the mean values and bootstrap standard errors for two pre-treatment covariates——respondents' type of childhood residence and their ethnicity—for compliers, always-takers, and never-takers. Since this method is only compatible with binary treatments, I dichotomized the treatment variable at the educational level at which the first stage is strongest in each country.[14] Across all contexts, the strongest effect of compulsory reforms was on increasing the probability of having eight or more years of education (See Table A6).

Descriptive Results

Expansion Reforms and Women's Schooling

Figure 1 provides an overview of women's educational attainment by birth cohort. Women in all countries have significantly increased their years of schooling over time. For instance, women born in the 1960s had an average of seven years of education in Colombia and Peru, and around

[13]Using the package ivdesc created by Marbacj and Hangartner (2020).

[14]I specifically considered the size, significance, and F-test of each estimate.

six years in the case of Bolivia. By the 1985 cohort, which was fully exposed to expansion reforms in all countries, women's mean years of schooling was ten years. While these improvements in women's education shortened the gender gap in schooling in Bolivia and Peru, women still attained fewer years of education than men—for the 1980 cohort, the gap was of 0.3 years in Bolivia and 0.4 years in Peru. In contrast, Colombian cohorts were already exhibiting a female advantage of 0.72 years of education (Author's calculations from IPUMS-International 2020).

[Figure 1 here]

Schooling and Women's autonomy across Cohorts

We now turn to examine the association between education and women's autonomy. Table 2 (Columns 1, 3, 5) contains the estimates of OLS models for each country between women's years of schooling and their autonomy in the home, pooled across cohorts. As expected, education does enhance women's autonomy in all countries. In particular, an extra year of schooling is associated with a rise in the autonomy scale, ranging between an increase of 0.021 (p-value < 0.001) to 0.006 (p-value < 0.001) standard deviations.

[Table 2 here]

To examine how the returns to schooling change as school expansion unfolds, I include a series of interaction terms between women's schooling and their birth cohorts (Table 2; Columns 2, 4, 6). Models contain grouped-birth cohorts born between 1970 and 1990, comprising women who were and were not exposed to these policies. For women born between 1970 and 1974, the omitted category, an extra year of schooling increases their autonomy in Bolivia, Colombia, and Peru. Nevertheless, interaction terms reveal that these positive returns decline in younger birth cohorts in all countries. In Bolivia, the magnitude of this decline is noticeable in the 1980-1984 birth cohort, for which the association between one year of schooling and women's autonomy

decreases in almost two percentage points (Column 2, p-value < 0.001) when compared to the 1970-1974 birth cohort. In the case of Colombia and Peru, interaction terms for the 1985-1989 birth cohorts are also worth noting. In the former, the decrease in the positive effect of schooling is 1.1 percentage points (Column 4, p-value < 0.001), while in Peru it is of 2 percentage points (Column 6, p-value < 0.001).

Consistent with prior observational studies, these results show that an extra year of schooling is associated with an increase in women's autonomy in Bolivia, Colombia, and Peru. However, we see that as educational systems expand, the positive returns of schooling for women's empowerment tend to diminish. Since these models are unable to control for individuals' unobserved characteristics and selection processes, we cannot solely attribute these results to women's schooling and its interaction with expansion reforms. In the next section, I use an instrumental variable approach to address endogeneity and provide a causal estimate of the effect of mass education on women's autonomy.

Main Findings

Compulsory Schooling Increases Education

What was the specific effect of the 1990s expansion reforms on women's schooling in Bolivia, Colombia, and Peru? Table 3 contains a series of OLS models regressing total years of schooling on cohort exposure to these policies. These models can also be referred to as first-stage estimates in the context of instrumental variables. We can see that expansion reforms significantly increased women's educational attainment in all countries. The effect was largest in Bolivia, where girls exposed to the reform had an average of 0.56 more years of education (p-value < 0.001) than unexposed students. Meanwhile, exposed cohorts in Peru increased their education in 0.47 years (p-value < 0.001), while exposed girls in Colombia gained 0.37 more years of schooling (p-value < 0.001). Given that the average schooling in all countries was already more than eight years, the

magnitudes of these effects are sizeable.

[Table 3 here]

Importantly, these results confirm that compulsory reforms in Bolivia, Colombia, and Peru are sufficiently strong instruments for examining how schooling affects women's autonomy. For instance, in all countries, reforms have a p-value lower than 0.001, t-values larger than 4.0, and an F-value of a considerable magnitude (Stock et al. 2002).

The Effect of Mass Schooling on Women's Autonomy

How does greater schooling impact women's autonomy? Table 4 contains the estimated effects of one extra year of schooling on the autonomy scale, as well as separated items. Before proceeding to these results, it is important to remember that IV estimates only apply to individuals induced to remain in school because of compulsory policies, in other words, those individuals that comply with the treatment.

In all countries, point estimates indicate a negative effect of one extra year of education, that is, exposed women have less autonomy relative to those unexposed to these reforms. These effects are statistically significant in Colombia and Peru. In particular, Peruvian women who attended school an extra year experienced a decrease in their autonomy of -0.37 standard deviations in the autonomy scale (p-value < 0.001), while in Colombia, a one year increase in education led to a decrease in women's autonomy of -0.10 (p-value < 0.001). In the case of Bolivia, point estimates also reveal a negative effect but not statistically significant effect of an extra year of schooling among compliers—— -0.66 standard deviations in the autonomy scale.

[Table 4 here]

We can take a closer look at these findings by focusing on the separate autonomy items

in Table 4 (Columns 2-5). Findings show that schooling has a consistently negative effect on women's control of major household purchases in all countries. These effects are significant in Peru—where one extra year of schooling has a negative effect of 25 percentage points (p-value $<$ 0.001)—and Colombia—where education has a negative effect of 12 percentage points (p-value $<$ 0.001). Schooling among compliers also negatively affects women's ability to decide about family visits. In the case of Peru, one extra year of education has a negative effect of almost 21 percentage points on decisions about family visits (p-value $<$ 0.001). In addition, women who attain one more year of education experience a decrease in their ability to decide about their health care. This effect is statistically significant in Colombia (1.7 percentage points p-value $<$ 0.057) and Peru (12 percentage points p-value $<$ 0.036). Lastly, schooling does not impact women's ability to make decisions about their money. It is important to note that the sample size for this question notably decreases in all countries due to non-response. The latter might be because many women do not have earnings of their own or because the question touches on sensitive topics.

Mass Schooling and Women's Background

Instrumental variables only allow researchers to make causal inferences about individuals who comply with the treatment, namely those women who continued their education due to compulsory schooling. Investigating whether this group differs from students who would have attended school regardless of these reforms (always-takers) or who did not comply with the treatment (never-takers), may be relevant to contextualize the main findings of this study. Following the approach developed by Marbach and Hangartner (2020), I examined whether compliers, always-takers, and never-takers differ in their pre-treatment characteristics, namely, their ethnic and geographical make-up.

[Figure 2 here]

Figure 2 shows that in Bolivia and Peru, a higher proportion of compliers lived in rural areas

compared to always-takers. Among women that continue their schooling due to compulsory laws, 45% in Bolivia and 52% in Peru lived in rural areas during their childhood. Meanwhile, only 13% and 19% did so among always-takers, in Bolivia and Peru, respectively. The proportion of students from rural backgrounds is even larger among those that did not continue their schooling (never-takers). These findings are consistent with prior studies showing higher drop-out rates among girls from rural areas in both countries. Nevertheless, these estimates also show that compulsory laws successfully kept girls from rural backgrounds in school. In Colombia, we do not see significant differences in the proportion of students from rural areas between compliers and always-takers, but we do between students who would have continued their schooling regardless (always-takers) and those who did not comply despite compulsory laws (never-takers).

Figure 3 shows the distribution of respondents according to their ethnicity. Although in Bolivia and Peru, the proportion of compliers from a minority background is higher than always-takers, these differences were only statistically significant in Peru. However, among never-takers, we do observe a larger proportion of indigenous or Afro-descendant women compared to always-takers across all countries.

[Figure 3 here]

This analysis suggests that compliers to compulsory reforms in Bolivia and Peru were different from women who would have continued their education regardless of these laws (always-takers)—they were more likely to come from rural backgrounds and being indigenous (in the case of Peru). In Colombia, however, differences between compliers and always-takers were less noticeable.

Given the relevance of having a rural background for treatment compliance, I also explore if they are heterogeneous effects of mass schooling according to respondents' residential context in childhood. Since only Bolivia and Peru had valid first stages[15], I only conducted this analysis for

[15]In Colombia, first-stage estimates do not yield evidence of expansion reforms as a strong instrument.

these two countries (See Table A7). These findings show that an extra year of schooling decreased women's autonomy for students that grew up in a rural locality, both in Bolivia and Peru (See Table 5). In particular, Bolivian women with an extra year of education experienced a decline of 0.13 standard deviations ($p < 0.05$) in their shared decision-making and autonomy, while in Peru the decline was of 0.06 standard deviations ($p < 0.05$). In contrast, an extra year of schooling did not significantly impact women's autonomy for those who spent their childhood in urban environments. These results further confirm that, at least in Bolivia and Peru, the treatment of compulsory schooling and its negative effects on autonomy were concentrated among women who grew up in rural contexts.

[Table 5 here]

Pathways: Women's Schooling and Union Formation

Because schooling may impact women's household formation by affecting their likelihood of entering a union and the type of partners they marry (Basu 1992), I now turn to explore these pathways. As shown in Table 6, I find that women with one extra year of schooling are more likely to be never married in all countries.[16] This marriage penalty is especially sizeable in Bolivia and Peru, where exposed women are 27 and 28 percentage points more likely to be never married. These estimates show that women who were exposed to compulsory reforms, are less likely to marry that those who were not.

[Table 6 here]

We can look at the group of women who are married in the sample to explore whether schooling also impacts their partner's characteristics, in particular their education and age. In the case of

[16] In 2000, the median age at marriage/cohabitation for women was of 22.6 In Bolivia, 23.1 in Colombia, and 23.6 in Peru (World Bank 2020). Similar findings were found when estimating this model only among women between the ages of 25 to 34. See Table A8.

Peru and Bolivia, we can see in Table 7 that exposed women are more likely to have hypogamous unions, in other words, they are more prone to "marry down" in education. This is the case when the difference in schooling is of one or more years (Column 3), and also when the gap is of three or more years (Column 5). Conversely, exposed Bolivian and Peruvian women are less likely to form hypergamous unions—marrying men with more schooling than themselves (Column 2 and 4). In the case of Colombia, we see the opposite pattern. Women with an extra year of schooling are more likely to marry men with more years of education (Columns 2 and 4). Lastly, no significant effects were found pertaining to the age of partners in any country.

[Table 7 here]

Finally, I also estimated the heterogeneous treatment effects of schooling on women's autonomy by marital status. Since relationship status is affected by respondents' education, as we just saw, these findings should be evaluated with caution. Table A9 shows that greater education has a positive effect on autonomy for unmarried women (either live alone or with parents) in Bolivia and Peru. In contrast, point estimates for married and cohabiting respondents are negative in all countries, although they are not statistically significant. That the negative effects of compulsory schooling appear to be concentrated among married respondents suggests that a compensatory dynamic may be driving the curtailment of women's autonomy among couples. To further explore this pathway, I examined whether married women exposed to compulsory schooling were more likely to experience controlling behaviors from their husbands. Estimates in Table A10 show that women's greater schooling increases these types of behaviors among partners, especially in Bolivia and Peru, where effects are sizeable but not significant. Together, these results suggest that the limitation of female autonomy in Bolivia and Peru was the product of a compensatory dynamic to re-affirm masculine authority when threatened by women's education, a dynamic documented in prior scholarship in Latin America (Menjivar 2011; Safa 1995).

Robustness Checks

I implement a series of checks to demonstrate the robustness of these results. First, to further assess the exogeneity of compulsory reforms, I conducted a series of t-tests comparing pre-treatment covariates between women that were exposed and not exposed to these policies. The majority of these tests show there are no significant differences across these two groups in terms of race/ethnicity and type of childhood residence, with a couple of exceptions, where the mean differences are minimal (See Table A11 in the Appendix).

Second, I test whether results are sensitive to the precise definition of exposed cohorts. Indeed, the correct delimitation of exposed and unexposed cohorts is paramount to ensure the plausibility that compulsory reforms affect women's autonomy only via schooling. On the one hand, cohorts intervals should be narrow enough to guarantee that exposed and unexposed groups are comparable and differ only on their level of exposure to compulsory schooling laws. On the other, especially narrow birth cohorts are problematic due to the need for sufficiently large sample sizes for the IV analysis.

In particular, I re-estimated first stage and second stage models changing the definition of exposed birth cohort, spanning from a very close window of only two birth cohorts above and below the age cut off, to a broader definition of exposure that includes five birth cohorts at each side of the cutoff. Tables A12 and A13 in the Appendix show that the main findings are robust to the precise definition of exposed birth cohorts. As expected, estimates from a tighter cohort window have larger standard errors, but importantly the direction of the effects is consistent across the different definitions of exposure. The results presented in the chapter use a three birth-cohort interval above and below the cutoff as a mid-point definition, consistent with recent work applying this analytical strategy (Behrman 2015; Weitzman 2018).

A related concern is that the use of calendar-year cut offs to define exposure to compulsory reforms, might lead to defining partially exposed cohorts as unexposed. The latter would occur in instances were students lag in grade attainment either due to repetition or late entry into primary

school. Both issues were common in Bolivia, Colombia and Peru, especially among rural populations (Psacharopoulos and Tzannatos 1993). In particular, country reports show that an important proportion of students in rural areas lagged between 2 to 3 years in primary school entrance (Ilon and Moock 1991; MINEDUC 2004).

To examine how partial exposure might affect these results, I re-define the unexposed group and exclude those cohorts that might have been partially exposed to compulsory reforms. More specifically, I estimate IV models comparing the outcomes of women born between 1977-1979 (fully unexposed) with those born between 1983-1985 (fully exposed) for Bolivia, women born between 1974-1976 (fully unexposed) versus those born between 1980-1982 (fully exposed) for the case of Colombia, and women born between 1976-1978 (fully unexposed) versus those born between 1982-1984 (fully exposed) for Peru. This definition assumes that the majority of partially exposed students are concentrated in the three birth cohorts born before compulsory reforms in each country. It is worth noting that selecting cohorts further away from the reform increasingly hinders the comparability of the treatment and control groups. Table A14 shows that results using this alternative definition of unexposed cohorts have the same negative direction and substantive interpretation as the main findings of this study. Note, however, that the magnitude of coefficients is lower across countries, and that in the case of Bolivia, estimates turn to be statistically significant in almost all outcomes, probably due to increases in sample size.

Lastly, another potential threat to the exclusion restriction is the existence of a secular time trend shifting family decision-making norms across cohorts. If this were true, my results attributed to extra schooling would be due to the existence of this over time shift in decision-making. To check if this was the case, I ran an alternative specification of the main models controlling for birth cohorts, as a proxy for time trends, in both the first and the second stage. Findings were substantively the same, revealing that these results do not represent a secular time trend that coincides with compulsory reforms (See Table A15 in the Appendix).

Discussion and Conclusions

Demographers and sociologists have long argued that mass education is a critical policy to socialize individuals into new ideas about family norms and gender relations in the Global South (Caldwell 1980; Inkeles 1969). In particular, mass schooling was expected to accelerate family change by increasing women's bargaining power in the home, enhancing their autonomy and decision-making. As expansion reforms unfolded, however, several limitations of mass education as a policy to raise women's autonomy became apparent. One of them was the prevalent view of women's education as challenging men's status in families and threatening hypergamy norms in contexts of high gender inequality. Scholars found that in these settings, educated women were less prone to marry, and those who marry down in education were more likely to experience higher stress in their relationships—including higher rates of intimate partner violence (Behrman 2019; Jewkes 2002; Vyas and Watts 2009). These findings lead to alternatively hypothesize that mass education may have adverse effects on women's autonomy in contexts where hypergamy norms are widespread.

In this study, I examine these claims by analyzing the effect of compulsory schooling laws enacted in Bolivia, Colombia, and Peru on women's autonomy. These three Andean countries implemented these reforms at similar development stages to increase educational attainment among rural populations, and in the case of Bolivia and Peru, reduce gender gaps in schooling. I contribute to prior scholarship by leveraging the timing of these reforms and identifying the causal effect of women's schooling on their autonomy later in adulthood. This strategy allowed me to disentangle the impact of schooling on women's autonomy from other unobserved factors, such as differences in respondents' upbringing and family context. In addition, I explored the role of population heterogeneity and union formation patterns as potential explanations for these findings.

My first set of analyses relying on OLS models indicate that education is positively associated with a higher degree of female autonomy in Bolivia, Colombia, and Peru. However, these models also show that the positive returns of women's schooling diminish as educational systems expand,

and a larger proportion of women are educated in each cohort. Indeed, the decreasing returns of schooling as educational systems grow has also been documented in the case of labor market outcomes and civic participation (Bol 2015; Horowitz 2015). Once endogeneity is addressed via instrumental variables, I found that the cohorts of women exposed to compulsory laws experienced a decrease in their autonomy and decision-making. The effect's direction is consistent across all countries, with statistically significant effects in Colombia and Peru. Importantly, IV results pertain to compliers of compulsory education laws and not necessarily to those who would have continued their schooling regardless of these reforms. In Bolivia and Peru, I show that compliers to compulsory laws were more likely to come from rural backgrounds or a minority background and that the decrease in autonomy was stronger among women from rural localities.

Subsequent analyses suggest that union formation patterns are an important pathway for explaining these findings, especially in the case of Bolivia and Peru. My results indicate that women exposed to compulsory reforms were significantly less likely to marry or cohabit in all countries. Among married women, I found that exposure to mass schooling impacted the type of partners they espoused. In Bolivia and Peru, women with further schooling were more likely to marry less-educated men, while in Colombia, they were slightly more prone to "marry up" in education.

In contrast to classic demographic and modernization theories, these findings reveal that mass education did not enhance women's autonomy. In fact, for vulnerable groups of women, extending their years of schooling resulted in the curtailment of their autonomy. Importantly, these results further confirm that social scientists should not assume that greater education is a proxy of female autonomy across contexts. I interpret these findings as being consistent with two non-exclusive explanations for compulsory schooling's inability to increase women's autonomy. One concerns the trade-off between school expansion and student selectivity. I found that women's schooling is positively associated with a higher degree of autonomy across all countries. Nevertheless, as school systems expanded and incorporated more vulnerable populations via compulsory laws, these effects increasingly diminished. As qualitative studies suggest, it may be that schools did not offer the proper learning conditions or materials to enhance women's autonomy among disadvantaged

students (King and Hill 1993; Ohsako 1997; Stromquist 2001), whose families and communities were already hesitant to keep their daughters in school (Ames 2013). A similar argument was made by Grant (2015) who found further schooling had no effects on age at first birth among compliers of Universal Primary Policies (UPE) in Malawi. Thus, these findings highlight the importance of examining the differential returns to schooling according to population heterogeneity, both in terms of individuals' pre-existing conditions and heterogeneous treatment effects (Brand and Xie 2010). The case of women's autonomy suggests that respondents that were less likely to continue their education in the absence of compulsory reforms—women from rural and indigenous backgrounds in Bolivia and Peru—perceived lower returns to schooling in this particular outcome. These changes in selectivity may be relevant to assess the effects of compulsory schooling for other demographic outcomes in developing contexts, such as fertility timing, age at marriage, among many others.

A secondary explanation relates to the interaction between women's education and union formation patterns. In particular, I argue that compulsory schooling in contexts where women's education is viewed as a threat to marital norms may generate the unexpected consequence of curtailing married women's autonomy. The fact that exposed women are less likely to get married in all three countries suggests that i) women are penalized in the marriage market due to their increased schooling and their potential threat to hypergamy norms, and/or ii) educated women purposely choose not to marry as a way to protect their autonomy. Moreover, I find that in Bolivia and Peru, exposed women who get married are more likely to "marry down" in education, violating hypergamy expectations. Unions in which wives have higher schooling than their husbands have shown to be problematic for women in a myriad of contexts. For instance, scholars have found that husbands are more likely to perpetrate intimate partner violence (Cools and Kotsadam 2017; Behrman 2019) and pursue other sexual partners (Hunter 2007; Morrell 2001). Following this literature, my results suggest that the higher restriction of women's autonomy in Bolivia and Peru may result from a compensatory response among couples that transgress educational hypergamy norms. Indeed, I found that the negative effects of compulsory schooling were concentrated among married respondents in Bolivia and Peru and that these subgroup of women were also more likely to

experience controlling behaviors from their husbands. Since I cannot causally test this hypothesis in this study, I invite future research to do so, especially as hypogamous unions are becoming more prevalent in developing contexts (Esteve et al. 2016; Lin et al. 2020).

Although my main findings are consistent across contexts, some differences between countries are important to note. The negative effects of compulsory schooling on women's autonomy and the marriage penalty experienced by exposed women were considerably smaller in Colombia. Moreover, in contrast to Bolivia and Peru, further schooling *increased* women's likelihood of marrying more educated men in Colombia. These differences suggest that Colombia's lower levels of gender inequality when mass schooling was implemented may have reduced the extent to which female schooling was viewed as threatening in families and unattractive in the marriage market. Despite that these countries shared similar socioeconomic conditions when compulsory schooling was implemented, what differentiated Colombia from Bolivia and Peru was the absence of gender gaps in education and relatively more gender-egalitarianism in families and the work-place. Future research should examine the precise role of gender disparities in education and other macro-level dimensions of gender inequality on the impact of compulsory schooling on women's status.

Several important caveats are necessary to qualify these results. First, this chapter focuses only on one particular dimension of women's status– their autonomy–and shows that for some vulnerable sub-populations, further schooling is an ineffective tool to enhance it. This study certainly cannot conclude that these women would have been in a better position in the absence of compulsory schooling—prior scholarship has demonstrated the myriad of positive effects of education for women's health, employment, and social mobility. Hence, it may be that further schooling did not enhance women's autonomy, but it may have improved their standing in other status dimensions. Second, this study only explored one set of pathways to explain these findings—changes in selection into schooling and the effects of women's education on union formation. However, prior research has detected other mechanisms, such as school quality and gender-biases in classrooms, that may further limit mass schooling's ability to increase women's autonomy. Exploring the role of these alternative pathways is a compelling area for future research. Third, because of data lim-

itations, the conclusions of this study pertain only to women. These results cannot explain how mass schooling affects men and their views about women's status. Because changes in women's autonomy in the home occur in relation to their partners, learning about the impact of schooling on men's gender-egalitarian behaviors is critical.

In closing, these findings are important for several reasons. Despite its limitations, this study was able to identify the causal effect of compulsory schooling on women's autonomy in Bolivia, Colombia, and Peru. Although there is a significant body of work examining the implications of schooling for women's empowerment in developing contexts, most of these studies do not address issues of endogeneity. My analytical strategy allowed me to identify how the positive returns of schooling for women's autonomy diminished as school systems expanded, and ultimately, how for vulnerable groups complying with compulsory reforms, further schooling did not fulfill the promise of empowerment. These findings highlight the importance of examining the differential returns to mass schooling considering population heterogeneity and the contextual meaning of women's education.

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Tables

Table 1: School Reforms by Country

Country	Date of Reform	Cohort Exposed	Changes in Mandatory Schooling
Bolivia	1994	1983-	From 5 to 8 years
Colombia	1991	1981-	From 5 to 10 years
Peru	1993	1982-	From 5 to 10 years

Table 2: OLS Models: Cohort x Education on Women's Status 1970-1990

	Bolivia		**Colombia**		**Peru**	
	Autonomy Scale	Autonomy Scale	Autonomy Scale	Autonomy Scale	Autonomy Scale	Autonomy Scale
Years Education	0.006***	0.016***	0.012***	0.022***	0.021***	0.029***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Birth Cohorts (1970)						
1975		0.023		-0.042**		-0.027
		(0.026)		(0.015)		(0.018)
1980		-0.052		-0.106***		-0.059**
		(0.030)		(0.016)		(0.020)
1985		-0.231***		-0.184***		-0.095***
		(0.053)		(0.021)		(0.027)
1990				-0.151***		-0.296***
				(0.029)		(0.057)
Cohort 1970 x Education						
1975 x Educ		-0.010***		-0.002		-0.002
		(0.002)		(0.001)		(0.002)
1980 x Educ		-0.019***		-0.005***		-0.011***
		(0.003)		(0.001)		(0.002)
1985 x Educ		0.009		-0.011***		-0.020***
		(0.005)		(0.002)		(0.003)
1990 x Educ				-0.030***		0.014**
				(0.003)		(0.006)
Constant	-0.047	-0.033	-0.070***	-0.122***	-0.371***	-0.372***
	(0.027)	(0.030)	(0.016)	(0.017)	(0.060)	(0.061)
N	14301	14301	76134	76134	83987	83987

* All models control for survey year and region. Bolivia and Peru models also control for ethnicity. Standard errors are clustered by survey cluster. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3: The Effects of Expansion Reforms on Years of Education (First Stage Estimates)

	Bolivia	**Colombia**	**Peru**
Exposure Reform	0.556***	0.467***	0.365***
	(-4.77)	(-8.46)	(-7.12)
Constant	9.735***	10.16***	8.154***
	(-31.18)	(-57.71)	(-16.65)
N	5796	22888	31644
F	18.14	40.35	103

* All models control for survey year and region. Bolivia and Peru models also control for ethnicity.

t-values in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4: IV Estimates for Women's Autonomy

	Autonomy Scale	Household Purchases	Visits	Own Health Care	Own Money
BOLIVIA					
Years of Education	-0.668	-0.462	-0.258	-0.273	0.227
	(0.454)	(0.306)	(0.181)	(0.191)	(0.838)
Constant	6.41	5.07	3.30	3.43	-1.28
N	4673	4673	4673	4672	2375
COLOMBIA					
Years of Education	-0.101***	-0.115***	-0.044***	-0.017	0.001
	(0.021)	(0.019)	(0.012)	(0.009)	(0.006)
Constant	0.89	1.56	1.13	0.93	0.97
N	22888	22888	22888	22877	15514
PERU					
Years of Education	-0.369***	-0.245***	-0.205***	-0.119**	-0.053
	(0.090)	(0.058)	(0.051)	(0.036)	(0.155)
Constant	2.85	2.53	2.34	1.66	1.47
N	27604	27604	27604	27592	14196

* All models control for survey year and region. Bolivia and Peru models also control for ethnicity. Standard errors are clustered by survey cluster. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 5: IV Estimates for Women's Status Scales: Urban vs Rural Background

	Urban	**Rural**
	Autonomy Scale	Autonomy Scale
BOLIVIA		
Years of Education	0.979	-0.126*
	(1.047)	(0.054)
Constant	-9.05	1.06
N	3263	1368
PERU		
Years of Education	15.336	-0.063*
	(115.501)	(0.032)
Constant	-134.14	0.43
N	17865	9699

* All models control for survey year, ethnicity and region. Standard errors are clustered by survey cluster. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 6: Pathways 1: Entry to Marriage

	Bolivia	**Colombia**	**Peru**
	Never Married	Never Married	Never Married
Years of Education	0.271***	0.124***	0.284***
	(0.052)	(0.016)	(0.038)
Constant	-2.632***	-0.771***	-2.110***
	(0.544)	(0.166)	(0.298)
N	5796	22888	31644

* All models control for survey year and region. Bolivia and Peru models also control for ethnicity. Standard errors are clustered by survey cluster. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 7: Pathways 2: Husband's Characteristics

	Homogamy	Hypergamy (+1 year)	Hypogamy (-1 year)	Hypergamy (+3 years)	Hypogamy (-3 years)	Husband's Age (+5 years)
BOLIVIA						
Years of Education	-0.408	-0.152	0.560	-0.444	0.908	0.467
	(1.139)	(0.394)	(1.413)	(1.079)	(2.338)	(1.402)
Constant	4.19	1.88	-5.07	4.51	-8.17	-3.95
N	3777	3777	3777	3777	3777	3421
COLOMBIA						
Years of Education	0.002	0.090^{+}	-0.092^{+}	0.076^{+}	-0.026	0.029
	(0.030)	(0.049)	(0.048)	(0.045)	(0.039)	(0.038)
Constant	0.23	-0.30	1.08	-0.40	0.84	0.28
N	12701	12701	12701	12701	12701	10497
PERU						
Years of Education	-0.017	-0.102^{**}	0.119^{**}	-0.049	0.115^{**}	0.066
	(0.036)	(0.035)	(0.040)	(0.030)	(0.046)	(0.041)
Constant	0.36	1.17	-0.53	0.64	-0.28	-0.04
N	23512	23512	23512	23512	23512	20864

$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors are clustered by survey cluster.

Figures

Figure 1: Women's Educational Attainment by Birth Cohort

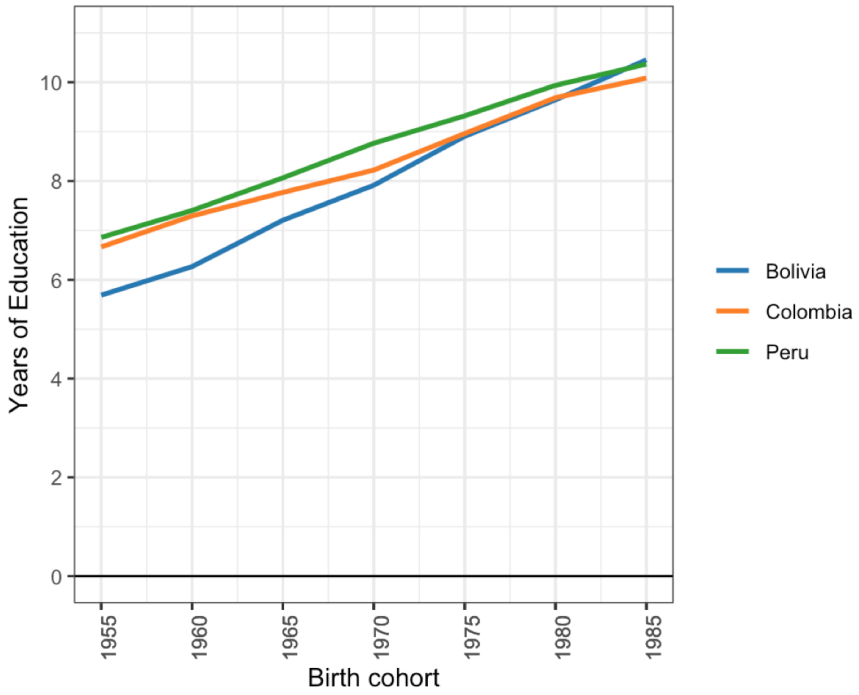


Figure 2: Proportion Rural Background

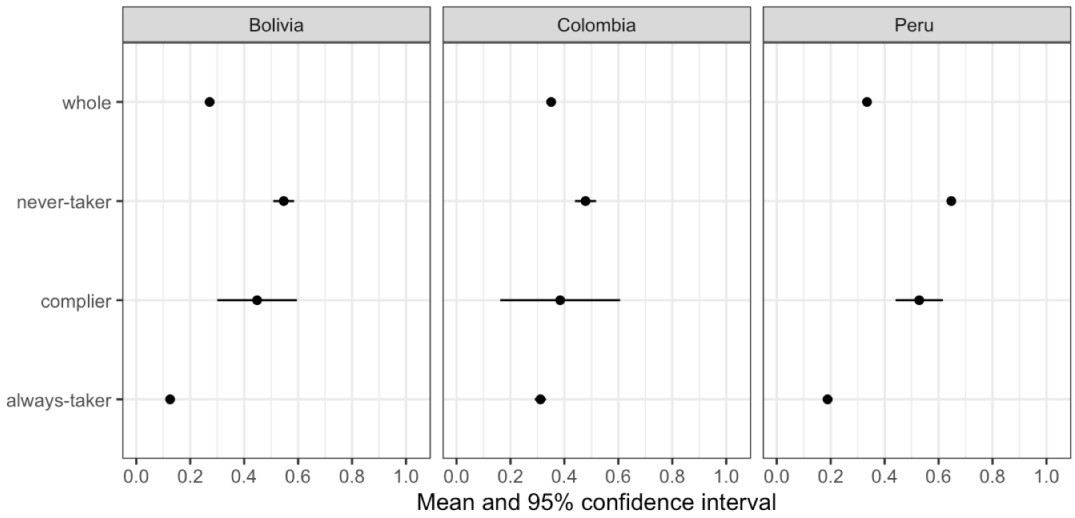
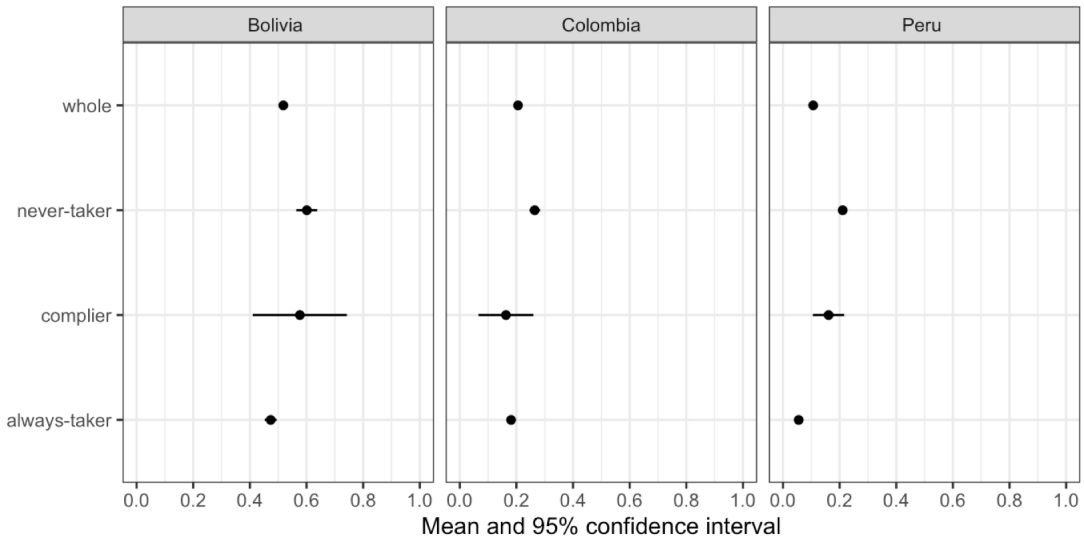


Figure 3: Proportion of Indigenous/Afro-descendants



Appendix Tables

Table A1: Country Statistics 1990

	Bolivia	**Colombia**	**Peru**	**Source**
General Indicators				
GDP per capita (current US$)	709.06	1,445	1,196.59	World Bank; 1990
Rural Population (% total)	44	31	31	World Bank; 1990
Female Labor Participation	41	39.1	37.8	World Bank; 1990
Human Development Index	0.54	0.6	0.62	UN; 1990
Mean Years of Education				
All	6.4	5.5	6.6	UN; 1990
Males	7.4	5.6	7.1	UN; 1990
Females	5.4	5.3	5.8	UN; 1990

Table A2: Descriptive Statistics Analytical Sample

	Unexposed			Exposed		
	Mean	**SD**	**N**	**Mean**	**SD**	**N**
BOLIVIA						
Years of education	9.46	4.68	3587	10.14	4.36	2289
Age at survey	24.41	2.56	3587	22.83	1.95	2289
Married/Cohabitant	0.63	0.48	3587	0.53	0.50	2289
Never Married	0.30	0.46	3587	0.41	0.49	2289
Separated/Widow	0.01	0.09	3587	0.00	0.06	2289
Position in the Home						
Head	0.10	0.30	3587	0.09	0.28	2289
Wife	0.47	0.50	3587	0.38	0.49	2289
Daughter	0.32	0.47	3587	0.40	0.49	2289
Daughter in law	0.04	0.20	3587	0.05	0.22	2289
Ethnicity						
White	0.47	0.50	3585	0.50	0.50	2288
Indigenous/Afro-descendent	0.53	0.50	3585	0.50	0.50	2288
Childhood Residence						
Rural locality	0.28	0.45	3552	0.26	0.44	2272
City or Town	0.72	0.45	3552	0.74	0.44	2272
COLOMBIA						
Years of education	9.15	4.28	11208	9.63	4.11	11680
Age at survey	30.62	4.91	11208	28.24	4.39	11680
Married/Cohabitant	0.65	0.48	11208	0.62	0.49	11680
Never Married	0.65	0.48	11208	0.23	0.42	11680
Separated/Widow	0.19	0.39	11208	0.01	0.11	11680
Position in the Home						
Head	0.18	0.38	11208	0.16	0.36	11680
Wife	0.50	0.50	11208	0.46	0.50	11680
Daughter	0.24	0.43	11208	0.27	0.44	11680
Daughter in law	0.03	0.17	11208	0.04	0.19	11680
Ethnicity						

White	0.79	0.41	6699	0.80	0.40	7428
Indigenous/Afro-descendent	0.21	0.41	6699	0.20	0.40	7428
Childhood Residence						
Rural locality	0.35	0.48	2599	0.35	0.48	2965
City or Town	0.65	0.48	2599	0.65	0.48	2965
PERU						
Years of education	9.67	4.35	15585	10.07	4.02	16061
Age at survey	28.51	2.57	15585	25.51	2.57	16061
Married/Cohabitant	0.71	0.46	15585	0.62	0.49	16061
Never Married	0.20	0.40	15585	0.30	0.46	16061
Separated/Widow	0.00	0.05	15585	0.00	0.04	16061
Position in the Home						
Head	0.09	0.29	15585	0.07	0.25	16061
Wife	0.54	0.50	15585	0.43	0.50	16061
Daughter	0.27	0.45	15585	0.37	0.48	16061
Daughter in law	0.03	0.18	15585	0.05	0.22	16061
Ethnicity						
White	0.89	0.31	15585	0.90	0.30	16059
Indigenous/Afro-descendent	0.11	0.31	15585	0.10	0.30	16059
Childhood Residence						
Rural locality	0.35	0.48	15563	0.68	0.47	16038
City or Town	0.65	0.48	15563	0.75	0.43	16061

Table A3: DHS Measures

Item	Wording	Response
1	Who usually makes decisions about visits to your family or relatives?	You, your husband, you and your husband jointly, you and someone else, or someone else?
2	Who usually makes decisions about health care for yourself?	You, your husband, you and your husband jointly, you and someone else, or someone else?
3	Who usually makes decisions about making major household purchases?	You, your husband, you and your husband jointly, you and someone else, or someone else?
4	Who usually decides how the money you earn will be used?	You, your husband, you and your husband jointly, you and someone else, or someone else?

Table A4: Descriptive Statistics: Main Outcomes

	Unexposed			Exposed		
	Mean	**SD**	**N**	**Mean**	**SD**	**N**
BOLIVIA						
Autonomy Scale	-0.01	0.70	3115	-0.05	0.74	1637
Household Purchases	0.66	0.48	3115	0.63	0.48	1637
Visits	0.81	0.39	3115	0.80	0.40	1637
Own Health	0.85	0.36	3114	0.82	0.38	1637
Own Money	0.93	0.25	1639	0.96	0.21	772
COLOMBIA						
Autonomy Scale	0.25	0.55	11208	0.22	0.57	11680
Household Purchases	0.71	0.45	11208	0.67	0.47	11680
Visits	0.85	0.36	11208	0.84	0.37	11680
Own Health	0.90	0.30	11203	0.90	0.30	11674
Own Money	0.98	0.13	7652	0.98	0.13	7862
PERU						
Autonomy Scale	0.09	0.66	13867	-0.02	0.72	13739
Household Purchases	0.68	0.47	13867	0.61	0.49	13739
Visits	0.81	0.39	13867	0.75	0.43	13739
Own Health	0.80	0.40	13862	0.76	0.42	13732
Own Money	0.97	0.16	7385	0.97	0.17	6813

Table A5: IV Estimates for Women's Autonomy without Regional Controls

	Autonomy Scale	Household Purchases	Visits	Own Health Care	Own Money
BOLIVIA					
Years of Education	-0.667	-0.481	-0.248	-0.270	0.867
	0.457	0.321	0.176	0.192	10.746
Constant	6.52	5.36	3.24	3.44	-7.74
N	4750	4750	4750	4749	2411
COLOMBIA					
Years of Education	-0.100***	-0.114***	-0.044***	-0.017	0.001
	(0.021)	(0.019)	(0.012)	(0.009)	(0.006)
Constant	0.81	1.45	1.09	0.93	0.98
N	22888	22888	22888	22877	15514
PERU					
Years of Education	-0.411***	-0.274***	-0.227***	-0.131**	-0.078
	(0.116)	(0.075)	(0.064)	(0.045)	(0.252 N
27604	27604	27604	27592	14196	
Constant	4.16	3.40	3.04	2.11	1.80

* All models control for survey year and region. Bolivia and Peru models also control for ethnicity. Standard errors are clustered by survey cluster. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A6: The Effects of Expansion Reforms on Years of Education by Grade (First Stage Estimates)

	Total Years	5+	6+	7+	8+	9+	10+	11+	12+	13+
BOLIVIA										
Reform	0.556***	0.063***	0.077***	0.083***	0.081***	0.077***	0.071***	0.067***	0.042**	0.012
	(0.117)	(0.009)	(0.011)	(0.012)	(0.012)	(0.013)	(0.013)	(0.013)	(0.013)	(0.012)
F	18	13	19	19	19	19	18	16	14	13
N	5796	5796	5796	5796	5796	5796	5796	5796	5796	5796
Constant	9.74	0.89	0.83	0.76	0.70	0.65	0.60	0.54	0.47	0.18
COLOMBIA										
Reform	0.467***	0.027***	0.054***	0.059***	0.063***	0.062***	0.057***	0.054***	0.037***	0.022***
	(0.055)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
F	40	39	50	48	42	41	35	32	22	18
N	22888	22888	22888	22888	22888	22888	22888	22888	22888	22888
Constant	10.16	0.93	0.86	0.82	0.77	0.73	0.68	0.63	0.31	0.25
PERU										
Reform	0.365***	0.033***	0.036***	0.057***	0.052***	0.050***	0.050***	0.041***	0.021***	0.009
	(0.051)	(0.004)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
F	103	34	43	106	113	123	122	122	81	74

N	31644	31644	31644	31644	31644	31644	31644	31644	31644	31644
Constant	8.15	0.83	0.77	0.53	0.52	0.47	0.44	0.43	0.23	0.18

All models control for survey year and region. Bolivia and Peru models also control for ethnicity.

Standard errors are clustered by survey cluster.

$^{*}\ p < 0.05$, $^{**}\ p < 0.01$, $^{***}\ p < 0.001$

Table A7: First Stage Estimates: Childhood Residential Background

	Bolivia Rural	Colombia Rural	Peru Rural	Bolivia Urban	Colombia Urban	Peru Urban
Reform	0.979***	0.334	0.633***	0.236	0.256*	0.118*
	(5.12)	(1.88)	(7.55)	(1.80)	(2.17)	(2.08)
Constant	6.774***	10.87***	5.676***	10.08***	10.71***	9.634***
	(13.76)	(16.72)	(7.14)	(32.98)	(26.86)	(20.93)
N	1552	1952	10575	4193	3612	21024
F	11.36	3.473	31.68	12.51	2.512	26.28

* All models control for survey year and region. Bolivia and Peru models also control for ethnicity. Standard errors are clustered by survey cluster. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A8: Pathways 1: Entry to Marriage (Ages 25-34)

	Bolivia	**Colombia**	**Peru**
	Never Married	Never Married	Never Married
Years of Education	0.149*	0.093***	0.155***
	(0.0687)	(0.0159)	(0.0224)
Constant	-1.361	-0.957***	-1.087***
	(0.695)	(0.183)	(0.175)
N	2227	13523	24950

* All models control for survey year and region. Bolivia and Peru models also control for ethnicity. Standard errors are clustered by survey cluster. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A9: IV Estimates for Women's Status Scales: Marital Status

	Married/Cohabiting	**Not Married/ Not Cohabitating**
	Autonomy Scale	Autonomy Scale
BOLIVIA		
Years of Education	-0.536	0.428*
	(1.865)	(0.188)
Constant	5.34	-4.95
N	3421	1101
COLOMBIA		
Years of Education	-0.029	-0.291
	(0.023)	(0.182)
Constant	0.44	2.92
N	14497	5088
PERU		
Years of Education	-0.041	0.575**
	(0.050)	(0.194)
Constant	0.42	-4.53
N	20866	5171

* All models control for survey year and region. Bolivia and Peru models also control for ethnicity. Standard errors are clustered by survey cluster. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A10: Pathways 3: Husband's Controlling Behavior

	Bolivia	**Colombia**	**Peru**
	Controlling Behavior	Controlling Behavior	Controlling Behavior
Years of Education	0.594	0.012	0.124
	(2.479)	(0.018)	(0.078)
Constant	-4.938	0.321**	-0.563
	(22.38)	(0.118)	(0.602)
N	2123	10143	18634

* All models control for survey year and region. Bolivia and Peru models also control for ethnicity. Standard errors are clustered by survey cluster. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A11: Pre-treatment Characteristics IV Analytical Sample

	Unexposed		**Exposed**		
	Mean	SD	Mean	SD	p-value
BOLIVIA					
Abroad	0.01	(0.09)	0.01	(0.09)	0.27
City	0.47	(0.50)	0.50	(0.50)	1.00
Town	0.25	(0.43)	0.23	(0.42)	0.08
Countryside	0.28	(0.45)	0.26	(0.44)	0.07
Spanish/White	0.47	(0.50)	0.50	(0.50)	0.96
Quechua	0.31	(0.46)	0.30	(0.46)	0.15
Aymara	0.17	(0.38)	0.17	(0.37)	0.22
Other	0.04	(0.19)	0.03	(0.18)	0.26
COLOMBIA					
Abroad	0.23	(0.42)	0.22	(0.41)	0.18
City	0.15	(0.36)	0.15	(0.36)	0.72
Town	0.35	(0.48)	0.35	(0.48)	0.33
Countryside	0.27	(0.44)	0.28	(0.45)	0.81
Spanish/White	0.79	(0.41)	0.80	(0.40)	0.97
Indigenous	0.10	(0.30)	0.09	(0.29)	0.02
Afro-descendants	0.10	(0.29)	0.10	(0.30)	0.70
Other	0.01	(0.12)	0.01	(0.09)	0.01
PERU					
Abroad	0.00	(0.03)	0.00	(0.04)	0.75
City	0.46	(0.50)	0.48	(0.50)	0.00
Town	0.19	(0.40)	0.19	(0.39)	0.34
Countryside	0.35	(0.48)	0.32	(0.47)	0.00
Spanish/White	0.89	(0.31)	0.90	(0.30)	0.99
Quechua	0.09	(0.28)	0.08	(0.27)	0.00
Aymara	0.01	(0.09)	0.01	(0.09)	0.31
Other	0.01	(0.11)	0.01	(0.11)	0.80

Table A12: First Stage by Number of Exposed Cohorts

Number of Cohorts	2	3	4	5
BOLIVIA				
Years of Education	0.396**	0.556***	0.581***	0.759***
	(0.140)	(0.117)	(0.109)	(0.102)
Constant	9.70	9.74	9.77	9.89
N	4074	5796	7461	9130
COLOMBIA				
Years of Education	0.339***	0.467***	0.566***	0.682***
	(0.066)	(0.055)	(0.048)	(0.043)
Constant	10.29	10.16	10.12	9.96
N	15512	22888	30255	37495
PERU				
Years of Education	0.230***	0.365***	0.447***	0.527***
	(0.062)	(0.051)	(0.045)	(0.041)
Constant	8.10	8.15	8.17	8.33
N	21413	31644	41556	50988

* All models control for survey year and region. Bolivia and Peru models also control for ethnicity. Standard errors are clustered by survey cluster. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A13: IV Estimates for Women's Status by Number of Exposed Cohort

Number of Cohorts	2	3	4	5
BOLIVIA				
Years of Education	-1.148	-0.668	-0.726	-0.510*
	(1.693)	(0.454)	(0.479)	(0.236)
N	3318	4673	5957	7160
Constant	10.75	6.41	7.02	5.01
COLOMBIA				
Years of Education	-0.090**	-0.101***	-0.115***	-0.123***
	(0.033)	(0.021)	(0.016)	(0.012)
N	15512	22888	30255	37495
Constant	0.72	0.89	1.09	1.18
PERU				
Years of Education	-0.449	-0.369***	-0.373***	-0.377***
	(0.235)	(0.090)	(0.067)	(0.054)
N	18696	27604	36034	43969
Constant	3.34	2.85	2.94	2.97

All models control for survey year and region. Bolivia and Peru models also control for ethnicity. Standard errors are clustered by survey cluster. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A14: Robustness Check: IV Estimates for Women's Autonomy No Partial Exposure

	Autonomy Scale	Household Purchases	Visits	Own Health Care	Own Money
BOLIVIA					
Years of Education	-0.383***	-0.241***	-0.160**	-0.140**	0.005
	(0.104)	(0.064)	(0.046)	(0.042)	(0.052)
Constant	3.83	3.07	2.41	2.21	0.95
N	4470	4470	4468	4470	2397
COLOMBIA					
Years of Education	-0.087***	-0.090***	-0.041***	-0.018***	0.001
	(0.010)	(0.008)	(0.006)	(0.004)	(0.003)
Constant	0.87	1.37	1.16	1.00	0.98
N	22203	22203	22203	22193	15085
PERU					
Years of Education	-0.263***	-0.195***	-0.139***	-0.077***	-0.019
	(0.037)	(0.026)	(0.021)	(0.015)	(0.012)
Constant	2.08	2.16	1.86	1.37	1.14
N	27339	27339	27339	27331	14210

* All models control for survey year and region. Bolivia and Peru models also control for ethnicity. Standard errors are clustered by survey cluster. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A15: IV Estimates for Women's Autonomy Scale Controlling by Birth Cohort

	Autonomy Scale		
	Bolivia	Colombia	Peru
Years of Education	-0.349	-0.118***	-0.316***
	(0.284)	(0.025)	(0.069)
Constant	3.50	1.06	2.45
N	4673	22888	27604

* All models control for survey year and region. Bolivia and Peru models also control for ethnicity. Standard errors are clustered by survey cluster. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Chapter 5

Conclusion

This dissertation examined whether recent increases in women's status in Latin America reduce long-standing gender inequalities in union formation and family dynamics. To do so, I analyzed two policies driving some of these shifts in the region—conditional cash transfer (CCT) programs, which drastically improved women's economic resources, and educational expansion reforms, which increased female schooling relative to men. My findings demonstrate that whether these transformations reduce gender disparities depends on the subgroups affected by these policies and how they interacted with existing cultural norms about gender and family.

The dissertation begins with the analysis of the Progresa program in Mexico—a case in which intra-marital status differences in economic resources were swiftly reduced when the State provided low-income mothers bi-monthly conditional cash transfers. Leveraging this program's experimental design, I demonstrate that these payments improved women's position in the home. In a relatively short time, these transfers shifted husbands' attitudes towards a more equitable housework division and improved wives' autonomy. Whether these effects were sustained over time

remains to be investigated, but ultimately this chapter shows that in some instances, "money can trump gender."

The growth of female schooling and its implications for union formation and women's autonomy is more complicated. In Chapters 3 and 4, I show that the interaction between women's education and the prevalence of hypergamy norms can lead to unexpected consequences. Specifically, I demonstrate in Chapter 3 that as gender gaps were reduced and/or overturned, gender asymmetries in union formation prevailed in Bolivia, Colombia, and Peru. Indeed, highly educated women were increasingly less likely to get married in all countries, hypergamous[1] couples augmented in Bolivia and Peru, and hypergamy propensities[2] persisted. Although I observed a rise in hypogamous[3] unions in Colombia—where women have had a female educational advantage for several cohorts—my analyses showed that these trends mainly were the product of compositional changes and not shifts in marriage propensities, further revealing the endurance of gender asymmetrical norms.

Chapter 4 examines how the rise of women's schooling affects their autonomy in adulthood by exploiting the timing of compulsory school reforms in Bolivia, Colombia, and Peru in the 1990s. My results demonstrate that the positive returns of schooling for women's autonomy diminished as educational systems expanded and a larger proportion of women were educated in each cohort. In fact, for women who entered the school system due to compulsory reforms, further education moderately decreased their level of autonomy. I argue this effect is driven by two mechanisms:

[1]Unions where husbands have more education than wives

[2]Defined as the social norms, gendered ideals, and individual preferences guiding the desirability or rejection of specific marital matches (Schoen and Kluegel 1988)

[3]Unions where wives have more education than husbands

changes in student selectivity during school expansion processes and "compensatory" dynamics among couples that deviate from educational hypergamy.

Taken together, this dissertation makes several contributions. First, it empirically tests established sociological theories about families and gender under different contextual conditions. For instance, most hypotheses about women's schooling and changes in marriage rest on the assumption that education increases women's economic power. However, in the Latin American context this link is weaker, suggesting it may be a plausible mechanism for the endurance of gender asymmetries in marriage found in Bolivia, Colombia, and Peru. Likewise, theories about the effects of economic resources on bargaining processes in the home were developed and tested in the case of women's labor market earnings. But to the extent that low-SES women rely on welfare income in many contexts, evidence based on women's earnings was not conclusive. Thus, this dissertation builds on prior scholarship by testing the scope conditions of established hypotheses, and by doing so, it refines and extends some of these approaches.

Second, uncovering whether increases in women's status have a causal effect on reducing gender disparities is critical from a theoretical and policy perspective. Because many studies in this area do not address endogeneity issues, which are frequent when analyzing marriage, mixed results are abundant in this literature. In this sense, attempting to approach causality when examining family dynamics can help to reconcile these disparate findings, moving the discipline forward into new questions. Two chapters of this dissertation contribute to this endeavor by applying different causal inference techniques. In Chapter 2, I use a design-based approach to causality, exploiting an existent randomized control trial, the Progresa program, which had one distinct feature enabling the examination of sociological hypotheses—cash transfers were provided solely

to women. In Chapter 3, I turn to observational approaches to causal inference by leveraging the timing of compulsory school reforms in Latin American countries. Although these analytical strategies are not exempt of other limitations, their main strength is to attribute specific outcomes to precise treatments, better connecting empirical evidence to existing theory.

Third, these findings have important implications for social policy. The reduction of gender inequality in low and middle-income countries has been an important goal for governments, and aid agencies often view it as a critical policy for development (Murphy-Graham and Lloyd 2016). Different initiatives, such as the promotion of girl's education, the provision of micro-credits, and the implementation of conditional cash transfers, seek to reduce these gender inequalities by improving women's material conditions and overall autonomy (Kabeer 2007; Moser 1989; Nagels 2016). This dissertation contributes new evidence about these policies' effectiveness by examining the case of conditional cash transfers and expansion reforms. For example, my results concerning the positive effects of conditional cash transfers for women's position in the household are relevant for current discussions about universal basic income and its potential benefits for gender equality. This work also shows that policy-makers should not assume that greater education is a proxy of female autonomy across contexts and for all groups. Indeed, Chapter 4 demonstrates that schooling does not fulfill the promise of greater empowerment precisely for those students coming from more gender unequal environments. Overall, analyzing these policies from a sociological lens can be especially useful to explore and understand some of these initiatives' unintended consequences.

I conclude this chapter by outlining two future projects that build on the findings of this dissertation.

Cultural Beliefs and Union Formation

Sociologists have had a long-standing interest in marital selection or "who marries whom" as a reflection of social class openness, racial hierarchies, and gendered ideals (Rosenfeld 2008; Schwartz and Mare 2005). In Chapter 3, I follow prior scholarship and used log-linear models to analyze changes in marriage propensities, net of changes in educational distributions. However, because marriage decisions are the product of individual preferences, norms, and opportunity structures, observational approaches, like log-linear models, cannot directly capture the normative beliefs guiding these processes (Zeng and Xie 2008). To address this challenge, I propose to investigate these beliefs via two survey experiments. Using a conjoint design, I will test several hypotheses in the literature theorizing the role of individual traits (England 2010; Rosenfeld 2008) and group boundaries (Blau et al. 1982; Merton 1941) in the process of partner selection.

The first experiment focuses on how the public rates the compatibility and stability of specific couples that differ in a series of traits. I focus on three characteristics—race, relative income, and relative education—to determine how differences between partners are relevant—or not—for shaping normative beliefs about marriage. A second experiment focuses on individuals' first-order beliefs on marital selection to understand how public beliefs relate to individuals' romantic preferences. That is to say, when thinking about their own partner selection, which traits respondents value the most in a potential husband/wife? Methodologically, the advantage of this experimental design is that it reduces social desirability and identifies the relative importance of multiple traits in a profile (Hainmueller et al. 2014). Previous work on marital trends, including this dissertation, has mainly focused on one dimension of assortative mating—education, income, or race—but in reality, partners choose each other based on multiple characteristics (Kalmijn 1998).

I plan to implement this project in the United States. The reversal of gender gaps in education

and the decline of marriage rates among disadvantaged populations (McLanahan 2004; Schneider et al. 2018) makes this country an interesting site to examine the role of gendered ideals and group boundaries. In fact, I already implemented the first experiment in a nationally representative sample in January 2021.

Educational Expansion and Family Change in Latin America

My findings regarding the reversal of gender gaps and marriage formation in Chapters 3 and 4 have lead me to several new questions. In Chapter 3, I found evidence of a retreat from marriage among highly educated women in Bolivia, Colombia, and Peru. Scholars have extensively studied similar patterns in East Asian societies, exploring whether these trends are explained by women's economic independence or marriage market mismatches. Building on this literature, I plan to further examine the mechanisms underlying this phenomenon in the Latin American case following two strategies. First, I will apply harmonic mean models (Schoen and Kluegel 1988; Raymo and Iwasawa 2005) to decompose these changes in marriage entry across cohorts and leverage cross-country variation in women's returns to schooling to assess the economic independence hypothesis. Second, I will further explore micro-level mechanisms via online survey experiments in selected countries. I am particularly interested in assessing what highly educated individuals perceive as "suitable" partners by applying a similar design as the one I proposed for the United States.

In addition, my findings regarding changes in marital sorting in Bolivia, Colombia, and Peru portend important implications for between-household inequalities in Latin America. The rise in homogamy propensities among the highly educated suggests that educational expansion may have exacerbated existing disparities in the region. Current research about assortative mating in Latin America has focused on describing these trends across countries (Esteve and McCaa 2007; Torche

2010), but have not studied the implications of these patterns for income inequality or related inter-generational processes. Thus, I am interested in pursuing these questions descriptively and causally by leveraging changes in compulsory laws that increased the likelihood of homogamy at different points of the educational distribution. Through this project, I expect to introduce the Latin American case in conversation with recent scholarship about the implications of assortative mating in low and middle-income countries (Pesando 2021; Shen 2021).

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